Non Official No. 6.
STORAGE

REPORT

OF

THE PROCEEDINGS

OF THE

METEOROLOGICAL CONFERENCE AT LEIPZIG.

PROTOCOLS AND APPENDICES.

TRANSLATED FROM THE OFFICIAL REPORT, APPENDIX TO VOL. VII., No. 24, OF THE ZEITSCHRIFT FÜR METEOROLOGIE.

Published by the Authority of the Meteorological Committee.

LONDON:
PRINTED BY GEORGE E. EYRE AND WILLIAM SPOTTISWOODE,
PRINTERS TO THE QUEEN'S MOST EXCELLENT MAJESTY.
FOR HER MAJESTY'S STATIONERY OFFICE.
PUBLISHED BY E. STANFORD, CHARING CROSS.

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Price One Shilling.

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INTRODUCTION.

GENTLEMEN,

HAVING received instructions from you on the 1st of July to attend the Conference of Meteorologists at Leipzig in the month of August, I beg to report that your instructions have been carried out, and I herewith submit to you a complete English translation of the Proceedings of the Conference, as published officially as an Appendix to the Journal of the Austrian Meteorological Society.

I have prefixed to the Report a translation of the Invitation, which appeared in the same Journal in the month of June last.

Your obedient servant,
ROBERT H. SCOTT.

To the Meteorological Committee
of the Royal Society.

Dec. 16th, 1872.

INVITATION

TO A

Meeting of Meteorologists to be held in Leipzig in August of this Year.

The development of interest in meteorological investigations in modern times among all civilized nations has brought into prominence a requirement which has been long felt, and to which utterance has often been given,* viz. that of greater uniformity of procedure in different countries. The undersigned accordingly venture to hope for general approval, if they allow themselves to make a few proposals which may tend to smooth the way for the ultimate assembling of an actual Meteorological Congress.

If there be any branch of science in which work on a uniform system can be especially useful and advantageous, that branch is the inquiry into the laws of weather, which, from its very nature, can only be prosecuted with a hope of success by means of very extensive observations embracing large areas, in fact, we might almost say, extending over the whole surface of the globe. Owing to the enormous amount of material with which the meteorologist has to deal, the necessity for improved facilities for summarizing facts is daily more and more felt, and these can only be afforded by increased uniformity in the methods of observation and publication.

Among the other results of the increasing interest in Meteorology of late years has been the introduction of new instruments and new methods of observation, the practicability of which, as well as the possibility of their general introduction into use, can only be determined by combined action.

It may furthermore be safely assumed that a personal inter-

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* Recently by Buys Ballot, in a special pamphlet, "Suggestions on a Uniform System of Meteorological Observations," Utrecht, 1872.

change of ideas among meteorologists will give an entirely fresh impulse to many branches of Meteorology.

It is only by means of an International Congress that speedy and certain methods of exchange of meteorological publications can be arranged—a step most urgently demanded, owing to the very nature of the science.

We think, however, that, without wishing in any way to anticipate the deliberations of the present meeting, it will be advisable, in order to fix ideas, to propose a certain number of questions which, in our opinion, may be proposed for discussion and possibly be answered.

1. Is it desirable to introduce the same Units of Measure (viz. of Length, Degrees, and Time) in meteorological inquiries in all countries, or is it sufficient to lay down certain fixed rules for the conversion of the different measures employed in the several countries?
2. What is the best form of Barometers for stations of the second order? Is the use of Aneroids for such stations admissible.
3. What mode of exposure of Thermometers for the observation of Air Temperature is the best and most suitable for general adoption?
4. What is the best construction of Maximum and Minimum Thermometers?
5. What instruments should be employed for determining the intensity of Radiation, and in what way can the comparability of the results obtained be ensured?
6. What instruments are the best for determining Earth Temperatures? At what depths should the observations be taken in order to attain the uniformity which is desirable?
7. What apparatus should be employed for the determination of the Hygrometric condition of the air? Are the Wet and Dry Bulb Thermometers sufficient? Can the Hair Hygrometer be employed, and under what restrictions?
8. In what way can uniformity in describing the Directions of the Wind be attained?

   Is the deduction of the mean Direction of the Wind by Lambert's formula desirable?

   Is it desirable or not to include very light winds (force 0) in constructing Wind Roses for the Direction of the Wind?
9. What scale should be employed for the Force of the Wind when it is determined, not by actual measurement, but only by estimation?

10. Is it desirable to introduce simple counting instruments for determining the Velocity of the Wind? What units should be taken for the discussion of the Velocity of the Wind?
11. What is the best form, size, and mode of exposure of Rain-gauges?
    At what hour of the day should the fall be measured?
12. Should the days of Rain and Snow be counted separately or together?
13. Is it desirable in giving the falls of Hail to draw a distinction between "Graupel"* and true Hail?
14. In counting Thunderstorms, should the Storms as such or the Days of storm be given? In what way should the instances of Sheet-lightning be noticed?
15. What apparatus is to be recommended for the measurement of Evaporation? What is the most suitable exposure for the Vaporimeter?
16. In what way should the proportion of Cloud in the sky be estimated and indicated?
    Is it desirable to introduce for Clouds, Hydrometeors, and for other extraordinary phenomena, a nomenclature which shall be independent of local language, and therefore universally intelligible?
17. Should other Meteorological Elements than those already enumerated, *e.g.* Atmospheric Electricity, &c., be included in the scope of normal observations, and what are the best instruments for observing them?
18. Can uniform Times of Observation be introduced for the normal observations?
19. Can rules universally applicable be laid down for the Verification of Instruments and the Inspection of Meteorological Stations?
20. What are the rules and what the intervals of time for which the Means of the several Meteorological Elements should be calculated? Which is the better plan, to begin the meteorological year with December or with January?
21. In what manner and for what intervals are the Normal Values for the individual Meteorological Elements to be deduced?
22. Is it desirable and possible to publish the Meteorological Observations of a limited number of stations in each country

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* There is no special term for "Graupel" in English: it differs from Hail, in that the stones are small pellets of snow, and are not coated with ice.—Trans.

in a uniform manner, and within a reasonably short time after the observations have been made?

23. How is the Exchange of the Meteorological Publications of different Institutions and different Countries to be carried out most speedily, safely, and simply?
24. Is it desirable that in each country there should be established one or more Central Institutions for the direction, collection, and publication of the Meteorological Observations?
25. Does the interchange of Weather Telegrams appear so useful that it should receive a fuller development and a firmer organization?
26. What Regulations should be adopted in order to carry into effect the Decisions and the Objects of the Meteorological Congress?

The enumeration of the preceding questions, the discussion of which will lead to other subjects for deliberation of the same character, should be sufficient to show the importance of the points which remain for settlement by an International Meteorological Congress, and we accordingly deem that we shall not be far astray if we count on the co-operation not only of all Meteorologists by profession, but also of Learned Societies, and, as an ultimate consequence, of Governments also, with regard to the realization of the idea of such a Congress and its practical efficiency.

We are of opinion that the actual Congress should be summoned for the autumn of 1873 and for Vienna. There are two reasons for selecting that city. Firstly, a new Central Meteorological and Magnetical Institute has been built, and will be fitted out with new instruments and in full activity next year; and, secondly, the International Exhibition, which will be open at the time, will form an additional inducement to foreign scientific men to visit Vienna.

For the present year we think it advisable to hold a preliminary Conference of meteorologists; of course this Conference can only be *consultative;* its chief duty will be to lay down the programme for the actual Congress, perhaps to instigate preparatory experiments on the questions proposed, and thereby to render it possible for the Congress to arrive at immediate conclusions on many points.

We venture by the present circular to invite the heads of Meteorological Institutes, the Meteorological and other Learned Societies, as well as private scientific men and practical observers in the domain of Meteorology, to this consultative meeting, which

is to be held in Leipzig at the time of the meeting of the Naturforscher Versammlung in that city. The gentlemen who are willing to take part in this meeting are requested to intimate their intentions at latest by the 13th of August to the undersigned Director of the University Observatory in Leipzig, Dr. Carl Bruhns. The commencement of the Conference is fixed for the 14th of August, and it is probable that the proceedings will last from the 14th to the 16th of August (inclusive).

All heads of Meteorological Institutes who do not take part in this consultative meeting, as well as the Meteorological and other Learned Societies which are not represented at it, are requested, in the interest of the cause, at least to communicate, by letter, their views and wishes in respect of the measures which should be adopted to ensure the uniformity which is desired, in order that the object may be attained with greater certainty by a combined effort, in which all take part.

(Signed) Dr. C. Bruhns.

Leipzig, May 1872.

(Signed) Dr. H. Wild.

St. Petersburg, May $\frac{15}{27}$, 1872.

(Signed) Dr. C. Jelinek.

Vienna, June 2, 1872.

List of Persons present at the Meteorological Conference at Leipzig.

1. H. Abich - - - Imperial Russian Staatsrath, Tiflis.
2. — Beez - - - Professor, Realschule, Plauen.
3. Dr. F. Behrens - - Privatdocent, University, Kiel.
4. Dr. G. v. Boguslawski - Stettin.
5. Dr. C. Bruhns - - Director of the Observatory, Leipzig.
6. A. Buchan - - Secretary, Scottish Meteorological Society, Edinburgh.
7. Dr. H. Buff - - Professor, University, Giessen.
8. Dr. Buys Ballot - - Director of the Royal Meteorological Institute, Utrecht.
9. Dr. P. Carl - - Professor, Munich.
10. Dr. E. Dreisch - - Halberstadt.
11. Dr. Ebermayer - - Professor, Aschaffenburg.
12. Dr. Feddersen - - Physicist, Leipzig.
13. W. H. v. Freeden - - Director of the Norddeutsche Seewarte, Hamburg.
14. L. Friedrichsen - - Cartographer, Hamburg.
15. Dr. Galle - - - Director of the Observatory, Breslau.
16. Dr. Hankel - - Professor, University, Leipzig.
17. Dr. J. Hann - - Assistant, Meteorological Central-Anstalt, Vienna.
18. Dr. Hänsel - - Professor, Realschule, Chemnitz.
19. Dr. C. Jelinek - - Director of the Meteorological Central-Anstalt, Vienna.
20. Dr. G. Karsten - - Professor, University, Kiel.
21. Dr. A. Krueger - - Director of the Observatory, Helsingfors.
22. H. Leppig - - Assistant, Observatory, Leipzig.
23. Dr. Lindemann - - Professor, Gymnasium, Annaberg.
24. Dr. E. Lösche - - Professor, Polytechnic School, Dresden.
25. — Mackendrick - - Edinburgh.
26. Dr. Meyerstein - - Göttingen.
27. G. J. Milne - - Edinburgh.
28. Dr. H. Möhl - - Professor, Gewerberschule, Cassel.
29. C. Moesta, Dresden - Late Director of the Observatory, Santiago, Chili.
30. R. Müller - - - Director of the Hydrographic Office, Pola.
31. G. Neumayer - - Hydrographer of the Admiralty, Berlin.
32. A. v. Oettingen - - Professor, University, Dorpat.
33. Dr. L. Pfeiffer - - Surgeon, Weimar.
34. Dr. M. A. F. Prestel - Professor, Emden.
35. D. Ragona - - Director of the Royal Observatory, Modena.
36. Dr. F. Ratzel - - Munich.
37. Dr. R. Rühlmann - - Professor, Gymnasium, Chemnitz.
38. Dr. G. Schenzl - - Director of the Meteorological Central Anstalt, Pesth.
39. C. Schildbach - - Physician, Leipzig.
40. Dr. H. Schoder - - Professor, Polytechnic School, Stuttgard.
41. Dr. P. H. Schreiber - Assistant, Gewerbeschule, Chemnitz.
42. R. H. Scott - - Director of the Meteorological Office, London.
43. Dr. E. H. Sell - - New York.
44. Dr. Selling - - Würzburg.
45. L. Sohnke - - Professor, Polytechnic School, Carlshruhe.
46. Baron O. v. Sternbach - Bludenz.
47. A. F. Sundell - - Physicist, Helsingfors.
48. Dr. O. Ule - - Editor of "Nature," Halle.
49. D. H. Weber - - Privatdocent, in Physics, Berlin.
50. H. Wild - - - Director of the Central Physical Observatory, St. Petersburg.
51. Dr. Wittwer - - Professor, Ratisbon.
52. Dr. J. Ziegler - - Frankfort-on-the-Maine.

PROCEEDINGS

OF THE

METEOROLOGICAL CONFERENCE AT LEIPZIG.

Protocol of the Meeting, 14th August, 12 to 4 o'clock, p.m.

M. BRUHNS opened the Conference and welcomed it in the name of the Saxon Government. Upon this M. D. RAGONA from Modena rose, and in the name of his countrymen expressed the wish that an international agreement on meteorological questions should be arrived at.

On M. BRUHNS' proposal the Conference chose M. BUYS BALLOT as President for this day; for the next two days Mr. SCOTT and M. WILD; for Vice-Presidents for the three days MM. BRUHNS, JELINEK, and WILD; as Secretaries, M. NEUMAYER of Berlin, and M. HANN of Vienna.

M. BRUHNS read the written replies, received to the questions proposed in the programme which had been distributed, from MM. Dove of Berlin, Mohn of Christiania, Wolf of Zurich, Hoffmeyer of Copenhagen, Pujazon of San Fernando, Fradesso da Silveira of Lisbon, Fritsch of Salzburg, Rubenson of Upsala, and Symons of London. And written expressions of sympathy had also been received from MM. Lauterburg of Berne, Heis of Münster, and Moritz of Tiflis.

M. BUYS BALLOT then opened the discussion of the programme and remarked that it was of great interest to have received in writing the opinion of some of the most eminent men in meteorological science.

The next point decided was as to the commencement and duration of the sittings. It was decided that they were to last from 12 till 4 o'clock, without interruption, and the meeting then proceeded to discuss the questions laid before it.

1. Is it desirable to introduce the same Units of Measure (viz. of Length, Degrees, and Time) in meteorological inquiries in all countries, or is it sufficient to lay down certain fixed rules for the conversion of the different measures employed in the several countries?

M. BRUHNS first remarked that it would be best to postpone the discussion of this question until they had received the result of the deliberation of the Metric Conference in Paris.

Uniformity of scales.

Mr. SCOTT thought that the Conference would lose time by this discussion of a subject which really could only be arranged by Governments.

Mr. BUCHAN expressed himself in favour of the introduction of the metric scale. He thought that an energetical recommendation of the metric scale on the part of this Conference would produce a good impression on the British Government. The matter was certainly important, but it ought not to be made a vital question. The general introduction of the metric scale had at present difficulties to meet with, and therefore this Conference should, for the present, content itself with recommending the publication of Tables for the Conversion of Scales, especially for Rain and Wind Velocity, and should recommend a system uniform in all respects; especially a recommendation should be made to Norway and Sweden to reduce their measures for rain, which are unknown to foreigners. He remarked that it should not proceed further than a recommendation.

Mr. SCOTT remarked that, as far as he knew, England was less inclined to adopt the metric scale now than formerly.

M. PRESTEL said that the scientific world had now arrived at a turning point in meteorology; the dynamical treatment of it had hardly begun. He recommended the general introduction of the metric scale, as being the only one which offers the great advantage of simple relations between the linear and cubic units of volume and weight. If it be wished to represent the changes of the weather in their causal relationship, and especially to submit to calculation the part which Aqueous Vapour plays in the atmosphere, no other scale is possible.

M. BUYS BALLOT expressed himself fully satisfied with Mr. Buchan's observations. The mutual conversion of the individual scales could still be effected and the practice in Meteorology might be the same as in Statistics, viz., that one unit of scale be chosen for the means; the original scale being preserved for the individual observations. He had for twenty years past reduced, for his Jaarboek, the most different scales to the metric scales without very great labour.

M. BRUHNS recommended that this expression of opinion should be published as such, and that the wish should be appended, that, according to M. Buys Ballot's proposal, the Results of observations, or the Means, should be published in metric scales, in addition to their original scales.

This proposal was accepted by the meeting.

Mr. SCOTT further drew attention to one advantage possessed by the Fahrenheit scale for Weather Telegraphy. The degrees are so small that it is only necessary to telegraph whole numbers, without materially interfering with the accuracy of hygrometrical determinations.

He thought that M. Mohn, although he had not so expressed himself in his letter, would agree with him on this point.

2. What is the best form of Barometers for stations of the second order? Is the use of Aneroids for such stations admissible? Barometers.

M. Buys Ballot thought there was hardly anyone who would declare himself in favour of the Aneroid for scientific purposes.

M. Prestel expressed a wish that the value of Aneroids for scientific observations should be discussed.

M. Jelinek recommended, for normal stations, Mercurial Barometers of the most careful construction, but thought that for stations of the second order, the Aneroid was not a bad instrument for the determination of differences in pressure, and it could also be of value on shipboard, as the usual marine barometer was too sluggish. The various defects of Aneroid Barometers may be fairly contrasted with certain difficulties in the handling and the suspension of Mercurial Barometers.

M. Ragona remarked that Aneroids bore carriage badly, and that therefore the comparison of them with Standard Barometers was unsatisfactory.

Mr. Buchan agreed with M. Jelinek's views. Aneroids might be employed for the comparison of Mercurial Barometers *for very short intervals of time*, as *e.g.* on the inspections of stations, and on board ship; without doubt perfect confidence could not be placed on these instruments, but occasionally Mercurial Barometers too get out of order. As it is easy to transport aneroids in order to compare them with other barometers and with each other, they should not be taken out of the hands of meteorologists.

M. Bruhns said that aneroids could not be accepted as scientific instruments, but might often serve for a control, *e.g.*, in case of errors of observation being made with mercurial barometers, &c. He wished that the questions should be referred to a committee.

Mr. Scott remarked that when we compare the two statements that the Aneroid is not a scientific instrument, but is an instrument for controlling others, we have a contradiction in terms. The Aneroid is an instrument whose corrections are constantly changing, and therefore cannot be used at all.

M. von Freeden agreed with the last speaker. Aneroids which give good readings at the mean value of pressure do not follow the great barometrical oscillations; in such a case they undergo permanent alteration in their corrections.

M. Wild: Aneroids should not be entirely rejected. They are at least good as interpolation and difference instruments, to supply gaps in observations which may always occur if the Mercurial Barometer is damaged.

M. Müller expressed himself against the use of Aneroids.

Mr. Scott remarked that the more costly Aneroids with thermometers were even less useful than those without thermometers, inasmuch as in certain cases of injury, the quicksilver of the thermometer destroys the whole instrument.

M. Ragona observed that stations of the second order might

Barometers.

very well be provided with instruments of that construction, and, although it was not perfectly satisfactory as a regular observing instrument, it could very well be used for occasional interpolations.

The PRESIDENT, M. BUYS BALLOT, summarised the result of the deliberations on the utility of the Aneroid to the following intent:—that the Aneroid should not be used *instead* of the Mercurial Barometer, but only as an interpolation instrument, *in addition* thereto.

M. BRUHNS proposed that the second point in the question under discussion; respecting the construction of a Barometer suitable for stations of the second order; should be referred to a committee, and suggested MM. JELINEK, HANN, and BUCHAN as members of it.

M. von OETTINGEN thought the matter too important to hand it simply over to a committee, and thought a decision should be come to at the present meeting; while M. WILD remarked that the appointment of a committee whose members lived in different countries was a bad arrangement.

Finally, M. HANN was requested to undertake alone the duty of reporting on this question, and to put himself in communication on the subject with foreign authorities.

Exposure of thermometers.

3. What mode of exposure of Thermometers for the observation of Air Temperature is the best and most suitable for general adoption?

M. RAGONA expressed himself against the enclosure of thermometers in a screen, and thought that readings taken outside a window were good and might well be adopted in case of the observers' employing a telescope for reading, as he always did.

M. BRUHNS requested Messrs. Scott and Buchan to give their opinions on the subject.

Mr. BUCHAN held the question as one of the greatest importance for the determination of temperature, because it included also that of the height of the thermometers above the ground. He thought that all the experience which had been gained on the subject hitherto had not as yet led to positive conclusions, but that a committee should be nominated to decide on the height of the thermometers, the size of the screens, and the necessary circulation of the air, so that it should be in a position to make definite proposals at the Meteorological Congress at Vienna next year.* In Scotland they used Stevenson's mode of exposure in a wooden box, four feet above the ground, and the soil below must be covered with grass.

Mr. SCOTT explained the construction of Stevenson's screen, and drew attention to some of the difficulties which are met with in the exact determination of air-temperature, the causes of which

* Correction in English edition. For "decide on"—"next year," read "collect "statistics from institutions and observatories, relative to the height of the ther- "mometers above the ground, and the methods by which they are protected, and "prepare a report for the Meteorological Congress at Vienna, as a basis for discussion "of the whole subject."—A. BUCHAN.

are to be found in the fact that local conditions are of too great importance. It is especially difficult to give proper protection against radiation. The Scotch screens are good, but heavy snow produces inconvenience. On the other hand, in his opinion, Glaisher's screen was as good as useless. He thought that Mr. Buchan's proposal could hardly be adopted, and finally drew attention to a long series of observations by Mr. Griffith which embraced 20* modes of exposure, the observations being repeated several times daily, and of which the results might soon be expected to be published. Exposure of thermometers.

M. Bruhns made the proposal that, at the central observatories, experiments should be made with various modes of exposure of the thermometers, but it was in the first instance most desirable that the several directors of organizations who were present should explain to the meeting the various modes of exposure at present adopted.

M. Wild first complied with this request, and explained the Swiss mode of exposure of thermometers, behind double cylinders of sheet-iron, pierced with holes, and with a louvred roof. But in this form they must not be brought too near heated walls, and must not be exposed to the sun; therefore in Russia they enclosed them with a double wooden envelope, because in winter care must be taken to guard against the influence of the walls of houses which are strongly heated from inside, and in summer against the sun's rays which strike on the north walls. Differences of temperature, to the extent of a degree, are found between sheet-iron screens so protected and those placed freely before heated walls.

M. Jelinek explained the exposure of thermometers in Austria. Although it is very desirable to give up the exposure immediately before windows, some attention must still be paid to the convenience of the observers, especially in the districts where the observers are volunteers, and undertake observations without pay. He further drew attention to the rotation thermometer (thermomètre fronde) recommended by M. Renou, which is to serve for the determination of the true temperature of the air.

M. von Oettingen remarked that in northern countries it was simply impossible to give up the exposure at windows. He said that it would be very difficult to introduce that which was theoretically the best, because so much depended on the good will of the observers.

M. Ebermayer stated that his Government had put him in a position, by giving necessary funds, to carry out strictly the demands of science in regard to the method of observations, a circumstance which was attended with special difficulties in his case, because he had to deal with the determination of the temperature in forests and on high trees. He could not but draw special attention to the fact that complete trustworthiness of observations could only be guaranteed by State support.

M. von Freeden had at first tried the Scotch screen, but med-

* Correction in English Edition. The real number is 10, not 20.—R. H. Scott.

Exposure of thermometers.

dling hands had constantly interfered with this mode of observation. Now, his thermometers are placed in a louvred screen on the south side of his house, in deep shade. The readings are taken from inside the house. Frequent check-observations made with thermometers placed so as to face the north, on the Prussian system, had confirmed the satisfactoriness of his method of observation.

M. RAGONA sketched the plan adopted by him, in which the thermometer is placed in an aperture made in a thick wall, which is closed on the inside by a window and has a pent-house roof over it on the outside. It is read with a telescope.

M. BRUHNS explained the mode of exposure before a window adopted in Saxony. This is, at the best stations, on the north side, generally between double walls or louvres. But at the forest stations the psychrometers often face east and west, and are read there while the sun is not shining on them. Care is also taken that the thermometer is placed before a room which is not heated, and at 1½ foot from the wall. At the observatory he had made observations at various heights above the ground, and great daily differences were found, while the yearly and monthly means agreed. He proposed, in order to investigate the question, to refer it to a committee consisting of Messrs. Scott, Buchan, and Wild.

M. BUYS BALLOT drew attention to the necessity of free circulation of air in the screen.

M. RÜHLMANN described his experiments to introduce his thermometers into a constant current of air, of moderate force, kept up by an aspirator. By such means the true temperature might be determined as accurately as possible.

Baron von STERNBACH spoke in favour of the mode of exposure of thermometers usually adopted in Austria. The usual interval between the thermometers and the wall, suppose 3 feet, excluded the influence of the latter. In this way he had found the same temperature on all sides of his house.

Messrs. WILD and SCOTT declared themselves ready to test some modes of exposure, but the latter drew attention to the fact that the Scotch method determined the temperature at a height of 4 feet, the Prussian at 12 feet, so that an immediate comparison of the result was impossible.

M. WILD remarked that the experiments might be made at the same heights, but with different screens.

M. BRUHNS proposed to test the Scotch, Russian, and German modes of exposure at the heights of 8 and 20 feet for the purpose of comparison.

Mr. SCOTT remarked that he hoped to be able to have an opportunity of carrying on experiments at Kew up to a height of 120 feet.

M. WILD declared himself ready to carry on experiments at different heights, and stated that at Pulkowa such observations had already been made on the ground, and up to the height of 70 feet.

M. von OETTINGEN stated his conviction that it would be

impossible to introduce the same mode of exposure at all stations, but he thought that it would be possible in the way suggested to reduce the observations to similar heights. Exposure of thermometers.

M. von FREEDEN said he could have observations taken up to the height of 100 feet; he wished to be allowed to make the observations, at the upper station, only at 10 and 4 o'clock.

M. BRUHNS wished to suggest to MM. Jelinek and Prestel similar observations.

M. JELINEK was afraid that these experiments would not lead to the results that were desired, for with respect to the observations which had been instituted on a uniform plan by M. Prestel in Emden and M. Fritsch in Vienna, the results had been discordant, so that there was fear of failure in this case too. The influence of the different localities to which the experiments must be adapted, exercises a disturbing effect on the results of the observations. Virtually the subject to be investigated should be the influence of various heights on thermometers attached to a pole erected at a distance from any houses.

M. PRESTEL explained his method of determining temperature at various heights and communicated the results, remarking that the thermometers which had been employed had been exposed freely and without screens. The temperature increased with the height, and most rapidly on days of thunderstorms. At that time the highest and lowest thermometers showed differences as great as 5°. Furthermore the increase was considerable with S.E. winds, but almost *nil* with N.W.

M. BUYS BALLOT limited the question proposed in so far as the only matter was to determine local temperature correctly, and therefore the observer had always to give the height of his thermometers and adhere to it. He referred to his "Suggestions," p. 14.

The Conference then declared itself satisfied that MM. SCOTT, WILD, JELINEK, and PRESTEL should undertake the investigation of the various modes of exposure, and the debate on this subject was closed.

4. What is the best construction of Maximum and Minimum Thermometers? Maximum and minimum thermometers.

M. WILD communicated the result of his experience on the value of the maximum thermometer. Of all maximum thermometers which are in use there is not one which is really of value, at least not for stations. All get out of order, even Geissler's, which, in other respects are the best. Metallic thermometers are pretty good if often cleaned. The minimum thermometer, for which, without hesitation, a spirit thermometer may be adopted, is better and is also of greater importance, and therefore he recommended its use.

M. MÖHL uses the maximum thermometer with two glass rods of different colours laid one behind the other.

M. EBERMAYER had gained the experience that in this appa-

Maximum and minimum thermometers.

ratus one of the rods dips in the quicksilver, and he recommended a maximum thermometer prepared by Greiner in Munich, which has proved permanently satisfactory.

M. Bruhns finds that none of the maximum thermometers can be trusted to remain in order. It is only the minimum thermometer which never fails in action.

Mr. Scott recommended in general Negretti and Zambra's maximum, and, on inquiry, he stated the price of this instrument was about a guinea. Casella's minimum thermometer with mercury as thermometric liquid is very beautifully finished, but its action is not quite trustworthy.

M. Buys Ballot remarked that he had got favourable experience of these thermometers.

M. von Freeden drew attention to the fact that, for each of these instruments, the proper slope must first be discovered by trial, in order that it may perform regularly; a remark which was confirmed by M. Behrens who had compared several at various temperatures at Kiel for Professor Karsten.

Several members of the Conference remarked that a serious objection to the spirit thermometer was found in the fact that the spirit evaporated and condensed in the upper end of the tube.

M. Ebermayer stated that this objection could be removed if the tube were, at its entrance into the bulb, inserted nearly up to the inner side of the bulb, as Greiner in Munich did when requested. According to his experience this arrangement entirely removed the objection which had been mentioned.

The President then gave a resumé of the discussion on Question 4, the general tendency of which was that the performance of the minimum thermometer was satisfactory, but that of the maximum not so to such a degree that a uniform construction of it could be generally recommended. The meeting then expressed their opinion that it would be very desirable to devise trustworthy maximum thermometers. It further expressed the opinion that the reading of the thermometric liquid in maximum and minimum thermometers should be constantly compared with ordinary thermometers, in order to be able to determine the corrections (which are changeable in the case of spirit thermometers).

The second sitting was fixed for 12 o'clock next day. Mr. Scott was chosen as President, and M. Wild as Vice-President. The first sitting closed at 4 o'clock.

2. Protocol of the Sitting of the 15th of August.

Mr. Scott, as President, opened the meeting with a few official communications, and presented to the Conference some new publications of the British Meteorological Office. Two letters had been received from M. Hildebrandsson, of Upsala, and M. Estreber, director of the Statistical Office at Costa Rica, at present in Berlin.

M. Neumayer made the following proposal: Inasmuch as, according to the course of the proceedings hitherto, and the brevity

of the time which is still available, it will be hardly possible to take up for discussion the immediate interests of Maritime Meteorology at the Conference, a committee had better be at once nominated, in order to prepare a proposal for the next day's meeting, which, if adopted by the Conference, might be embodied in its Proceedings. M. NEUMAYER spoke at some length in favour of his proposal, and pointed out especially that, since the Brussels' Congress, nothing more had been done with a tendency to to establish uniformity in meteorological observations at sea. It was also especially advisable to come to an agreement about the division of labour, by which a prodigious saving of labour might be effected. The proposal was supported and accepted, and the following members of a committee were nominated: MM. NEUMAYER, SCOTT, BUYS BALLOT, BUCHAN, von FREEDEN and MÜLLER. Maritime meteorology.

5. What instruments should be employed for determining the intensity of Radiation, and in what way can the comparability of the results obtained be ensured? Radiation.

M. WILD proposed to request Mr. Symons to give a report to the future Congress on the modes of observations adopted in England for Radiation.

M. JELINEK submitted that English physicists might be requested to institute experiments and to give a report on the subject. Perhaps also physicists in Germany might be interested in it. He himself would be ready to institute experiments on the subject with the various forms of apparatus which were at the Central-Anstalt.

M. BUYS BALLOT pointed out the importance of these observations on Solar Radiation and its changes according to place and time.

M. SCOTT gave a report on the results of English observers, and stated that the difficulty consisted in the fact that the results obtained with different instruments were not comparable with each other, and one hardly knew with the instruments now in use, what was actually observed.

6. What instruments are the best for determining Earth Temperatures? At what depths should the observations be taken in order to attain the uniformity which it desirable? Earth temperatures.

M. WILD remarked that the same course of action should be recommended for that question as for the preceding one, viz., to request a report (from an expert) for the next Congress.

Mr. SCOTT called on Mr. Buchan to give his experience on the subject.

Mr. BUCHAN explained the arrangement for the determination of Earth Temperature in Scotland. The Scottish Meteorological Society has observed Earth Temperature for 16 years (since the end of 1856). Thermometers were placed at depths of 3, 12, and

Earth temperature.

22 inches in the ground, but as these observations were in the first instance of interest for Agriculture, and it was only in the second instance that they were of Meteorological importance, special attention had been paid to those at 3 inches deep. Further investigations were certainly necessary; as yet the results were small. In conclusion he drew attention to the observations of Professor Forbes, which had been published by Professor Piazzi Smyth, and contained some extremely interesting results.

M. von OETTINGEN requested that the discussion of this subject should not be closed, but that attention should be paid to a few matters which had special relation thereto. It certainly did not concern meteorological observations to determine the dependence of Earth Temperature upon the nature of the soil, and so it would be important to consider the proposal that all such observations should be made in soil of the same character.

M. EBERMAYER gave a report of the observations of Earth Temperature instituted by him on Lamont's method. He recommended that procedure very strongly, on the ground of comparisons which he had made with the earth thermometers by Greiner of Berlin, which differed about one degree from Lamont's. He drew a sketch of his arrangement of the observations, which could be seen in some detail in his book "Die physikalische Ein-" wirkung des Waldes auf Luft und Boden," to be published by Krebs at Aschaffenburg.

M. BRUHNS spoke in favour of the adoption of electrical thermometers besides Lamont's, but would have observations made at at least 12 stations, and depths of 0 to 3 metres. He hoped to be able to submit some results at the Vienna Congress.

M. NEUMAYER stated that he also had investigated Earth Temperatures at Melbourne in an elaborate manner, at the surface and at depths of 14, 31, 65, and 100 inches. The readings were taken every three hours. For the arrangement of the results he referred to his publication, "Discussion of the Meteorological and " Magnetical Observations made at the Flagstaff Observatory, " Melbourne, during the years 1858–63." Mannheim, 1867.

M. WILD, with reference to the lateness of the hour, proposed another mode of procedure, in order to bring all the questions under consideration. He thought that the more important questions, 18–25, should be first taken up.

M. von OETTINGEN supported the proposal, and it was accepted by the meeting.

The discussion of Question 18 was then taken up.

Uniform hours of observation.

18. Can uniform Times of Observation be introduced for the normal observations?

M. BRUHNS was of opinion that the fixing of the daily hours of observation should be left free to the separate countries, but it should be the duty of central stations to reduce the observations made at different hours in a uniform manner to true

means. If, however, even the hours of observation could not be settled uniformly, a minimum of observations might be laid down, and on this head he thought that he must propose that one daily observation must be considered insufficient. Two observations, taken nearly at the hour of the daily extremes of temperature, might certainly be sufficient. For three observations a day, the equidistant hours 6^h, 2^h, and 10^h, were the most advantageous. Uniform hours of observation.

M. JELINEK opposed the exclusion of single daily observations. If the stations were very close together, daily observations at one fixed hour were sufficient, in order, in connexion with the central observatory in the neighbourhood, to be able to study local climates, and to afford means for studying the changes of the phenomena of weather. On the other hand, the only stations the observations of which should be published were those where at least three observations a day were taken. This proposal was also supported by M. von OETTINGEN.

Mr. SCOTT summarised the suggestions which had been made. He thought that an agreement on the hours of observation could never be attained, and thought that any rule which aimed at that brought serious dangers with it.

M. BUYS BALLOT was of the same opinion. He recommended Dove's division of hours 6^h., 2^h., and 10^h. In his "Suggestions" he had already drawn attention to another question, which was whether the observations were to be taken according to local time or astronomical time, that is the time of a definite meridian. For the barometer he thought that, in order to obtain actual simultaneity, the introduction of the second time, called by him for shortness "astronomical," was desirable.

M. von OETTINGEN declared his consent to this proposal, and was of opinion that five or six central stations should observe according to astronomical time.

M. BRUHNS asked for an explanation as to how the temperature readings should be managed, which must still be carried on at the same time as the barometer observations. He could not see why the local time should be avoided, because, from self-registering records, the data which might be desired might be extracted for any moment.

M. BUYS BALLOT explained his views by showing that for magnetic observations absolute simultaneity was required, and yet that the reduction of automatic records, which were only given at hourly intervals, could not be used in the case of many investigations, such as inquiries into disturbances, &c.

M. BRUHNS stated that he agreed to a limitation to magnetic observations.

M. JELINEK remarked that each country, according to its extent, might have one or more stations with self-registering instruments and thereby, at least for the meteorological elements, every requirement for simultaneous data would be complied with.

Mr. SCOTT communicated his experience with the seven English observatories fitted with self-registering instruments. At these there were found cases of sudden and transient changes in the

Uniform hours of observation.

meteorological elements which disappear in the reduction of the records for the interval of an hour. As a proof of what he said he pointed out a special case from the Quarterly Weather Report which took place at Aberdeen on the 16th January 1869.

M. von Oettingen asked if, for instance, for equidistant hours of observation eight times a day, such as he had introduced at Dorpat, a conformity in the hours chosen were desired. His hours were 1^h, 4^h, 7^h, &c.

Mr. Scott asked the meeting whether conformity in this direction were requested.

M. Wild remarked that an agreement on simultaneous hours of observation brought with it the hateful question of the first meridian.

M. Bruhns thought however that a proposal with regard to the time at which frequent equidistant observations should be instituted daily was still desirable.

M. von Freeden remarked that on ship-board observations were taken at the even hours. It would be well therefore to introduce this plan for land observations.

M. Buys Ballot stated his opinion that the choice of the hour of observation was immaterial; the important matter was that the observations should be capable of combination, so as to give a daily mean, which would only require a small correction to give a true mean. He attached importance to the necessity of dividing the observations fairly, so that we should not have too many by day and too few by night. The deduction of means from combinations in which the day observations are in decided excess (as in Italy) was, in his opinion, faulty.

M. von Oettingen remarked that the reason that he chose the odd hours was because they were more rarely used for observations. It seemed to him desirable that observations should be provided for these hours.

Mr. Scott remarked that the observations at sea were not quite so regular as was generally assumed. Thus, for instance, whalers would not confine themselves to fixed hours.

M. Bruhns took up M. von Oettingen's proposal in order to obtain a decision on it. Should the hours of observation run from 0–23, or from 1–24?

M. von Freeden was in favour of 0^h as a commencement.

M. Jelinek said a distinction must be drawn according to the object for which the observations are used, *e.g.*, for the reduction of means it was immaterial whether you began with an even or odd hour. But it was not so with regard to the comparability of the individual observations. An agreement in this latter direction was desirable.

M. von Oettingen asked whether the day should begin with noon or midnight. MM. Bruhns and Wild, for instance, began the meteorological day at 1^h a.m. There was another question, how you were to deal with the Wind Velocities. Here you have to do with sums; how should you choose the intervals?

Mr. Scott remarked that in the Quarterly Weather Reports

the meteorological day began with 0^h (noon), and the sums of the velocity of the wind were taken from $23\frac{1}{2}^h$ to $0\frac{1}{2}^h$, $0\frac{1}{2}^h$ to $1\frac{1}{2}^h$, &c., so that the centre of these intervals agreed with the period of observation. Uniform hours of observations.

M. von FREEDEN observed that at sea the day was counted from 0^h to 0^h.

M. BUYS BALLOT said that the observations made on the same day could not be separated from each other.

M. BRUHNS proposed that, as opinions were so very much divided at present, it would be better to postpone this question till the Meteorological Congress next year, and with it the question whether it would not be good to introduce generally the astronomical division of time, in order to bring the continental observations into harmony with the English. The meeting agreed thereto.

19. Can rules universally applicable be laid down for the Verification of Instruments and the Inspection of Meteorological Stations? Verifications and inspections.

M. WILD made a proposal that the Conference might adopt it as a principle that meteorological stations should be inspected at certain periods, so that at least every five years, or certainly every 10 years each station should be inspected at least once. First of all there must be an accurate verification of instruments at the central observatories.

M. BRUHNS stated that when the Saxon organization was established he had obtained means for a yearly inspection of stations, but after the first three years he had found that an inspection at such short intervals was no longer necessary. For the barometer, inspections within short periods were always desirable. It would however be impossible at this meeting to determine the period at which an inspection should be made, all that could be done was to express a wish that an inspection should be made as frequently as possible.

Mr. BUCHAN held it particularly desirable to make an inspection at least every two years. He did not attach so much importance to the testing of the instruments as to the advantages arising from personal contact with the observers. This opinion should be expressed by a competent authority. The instruments should be verified at the place of observation, and the Rain-gauges in particular should be well looked after.

Mr. SCOTT communicated his experience on his mode of inspecting the stations. Admiral FitzRoy had never visited his stations. Mr. Scott found that for stations which sent in Daily Telegraphic Weather reports, it was not necessary to take instruments on the inspection. The daily comparison of the reports which were received by telegraph, permitted you at once to detect any change which had taken place in the instruments. Kew barometers will perform perfectly well for 8 to 10 years. A spare barometer was placed at each station, in addition to the

Verifications and inspections.

observing barometer. Every instrument was first tested at Kew and the correction determined. If, when he visited a station, he found that the difference between the two barometers had remained constant, he had sufficient security that no change had taken place in the observing instrument.

M. Bruhns always took normal instruments with him on his inspection, and compared them with the instruments at the stations. He found that hanging up the barometer obliquely between the times of reading was not satisfactory for its preservation, because air gradually got into the Toricellian vacuum. Since this method had been given up the condition of the barometers was much more constant. For the thermometers which were given out to the stations, the limit of error was $\frac{1}{20}$° Reaumur. For the barometers he thought that a correction as much as 0·1 millim. might be admitted.

Mr. Scott remarked that he did not compare the barometers at the stations with a standard instrument. If the two barometers at each station agreed to within ·005 inch, after the constant correction had been applied, he was content. If the difference were greater the barometers were changed. Three thermometers, previously tested at Kew, were issued to each station. If errors exceeding 0°·3 F. were found in new thermometers they were rejected.

M. Wild asked M. Bruhns if, when the errors exceeded the limits which had been assigned, he applied corrections or rejected the readings altogether.

M. Bruhns answered that in the latter case he exchanged the instrument either at the inspection or soon after, and therefore he always took instruments with him on his journey for this purpose.

M. Wild remarked that he gave the greater corrections of the instruments to be applied at the stations.

M. Jelinek said that he could not lay down any fixed limits for the dependability of instruments which should be sent to the stations, because he had always to contest with difficulty in obtaining funds for providing the instruments. For barometers, in general, a correction of 0·2 millim. was considered admissible, for thermometers 0°·1 to 0°·15 C. It was desirable also to satisfy yourself occasionally that no change had taken place in the normal instruments.

M. Neumayer said that at the establishment of the meteorological stations in Victoria, it had been assumed as a principle to inspect all stations and compare the instruments, and the observations of the stations the instruments of which had never been compared had not been published. Moreover, the directors of the chief observatories in Australia had made arrangements for the mutual comparison of their instruments: Melbourne, for instance, with Sydney, Hobarton and South Australia. It was a much more difficult thing in those countries to carry out the inspection; but still they thought it their duty to lay down the principle, but could not bind themselves to a definite time.

M. von Oettingen remarked that it was a very difficult

thing to make standard thermometers. In his paper on the calibration of thermometers he had drawn attention to the fact that there was a small error to be avoided in Bessel's method, the correction for which he had given. By means of his investigations on this subject he had attained a great facility in calibration, and he offered to calibrate a large number of thermometers if the instruments were sent to him; an offer which was accepted with thanks by the directors of meteorological institutes who were present. Verifications and inspections.

M. Wild was of opinion that even the careful calibration of standard thermometers was not sufficient to secure comparable results. Two different thermometers, accurately tested at their fixed points and carefully calibrated, could, according to Regnault's investigations, differ to the extent of 0°·2 or 0°·3 at 50° C., owing to the difference in the expansion of the glass. He proposed to test the Standard Mercurial Thermometers at all the Central Institutions with each other, and to compare one of them at least with the Air Thermometer, so as to be able to refer to the latter all statements as to temperature. At 50° C. the difference between the Mercurial and the Air Thermometer might be 0°·5, and at very low temperatures still greater differences might be expected.

M. von Oettingen, on the contrary, maintained that the greatest correction at 40° was not above 0°·26. He thought that it was remarkable that the boiling point of the thermometers remained constant, but not so the freezing point, as is well known.

M. Bruhns requested MM. Wild and von Oettingen to give a report on this subject to the Vienna Congress, a request which was supported by the meeting.

Mr. Scott remarked that not more than two or three points on the entire thermometrical scale were known with sufficient accuracy. He further observed that delicate thermometers might be sent to stations in the following way:—the thermometers should be first put in a suitable thick indiarubber tube, and then packed in a box. By this means instruments can be sent without injury even to extra European countries.

Mr. Buchan then expressed his readiness to give a report on the regular inspection of the Scotch meteorological stations, and as a result of the deliberations of Question 19, a resolution was adopted by the meeting that—

> "It was desirable to make a periodical inspection of the stations of each system as frequently as possible."

20. What are the rules and what the intervals of time for which the Means of the several Meteorological Elements should be calculated? Which is the better plan, to begin the meteorological year with December or with January?

M. Prestel spoke for December as the beginning of the year, and illustrated it by the yearly course of temperature at

Calculation of means.

Emden, which ought to suit the whole of Europe. There are only civil and no scientific reasons for beginning the year with January. He further drew attention to the yearly course of barometrical pressure at Emden, which also agreed with a yearly period, beginning with December.

Mr. Buchan gave his opinion for January as the beginning of the year, and remarked that in the Shetlands and Orkneys the minimum temperature occurred in February, and even in March. If any reason could be brought for a definite epoch for beginning the year, that epoch would certainly be January. At sea the temperature agreed on the whole with the conditions in Scotland, therefore M. Prestel's remarks were not valid.

M. Hann also drew attention to the fact that if it was intended to fix the beginning of the year for universal adoption, you should not start from the course of the meteorological elements in this climate, because this course might be quite different in different parts of the globe.

Mr. Scott remarked that he began the year with January.

M. Buys Ballot was for December as the beginning of the meteorological year, because that in that case, in each yearly volume, the information for all the four seasons might be given by combining the months in threes, consecutively, while in the other case the observations of the winter must be divided between two volumes.

M. Wild remarked that it made no difference with what month you began the year, for there was no period for this which had any physical basis, it was therefore best to follow the Civil Calendar, for by this means you preserved a close relation to common life.

M. Jelinek said it was immaterial, for the means, where you placed the beginning of the year; it was only for single years that the question had any importance. Moreover, most systems of observations began the year with January.

M. Prestel urged the importance of Seasonal means for the purposes of agriculture. It was very necessary to pay attention to these requirements.

M. von Freeden remarked that the Prussian system of observations, as one of the oldest, had adopted December as the beginning of the year, chiefly in order to suit itself to the requirements of the agriculturists, for whose benefit the organization had been established.

M. von Oettingen was not disposed to agree with the mode in which discussion of the present question had been taken up. The first point which ought to have been begun with was the settling of the first subdivisions of the year. A decision on this question involved that about the commencement of the year. After the Daily Means, the natural units for meteorological periods, we came to the Five-day Means, the combination of the days in fives, the only equi-distant periods in the year. If it was wanted to have Normal Values for the meteorological elements, first and foremost Daily Means were not suited for this, for there was no hope for eliminating the disturbances in less than a century. The Monthly

period, however, would be too long for this, and the Monthly period had no meteorological basis, and was a mistake, owing to its varying length. Here, then, we have the Five-day Means as a solution of the difficulty; for the Five-day means, a period only one twenty-fifth as long was sufficient, in order to determine Normal Means with the same probability as the Daily Means. The Monthly question might be entirely set aside, and the year divided into 73 Five-day periods, and the only thing to be decided was the day on which the first of these should be begun. Calculation of means.

M. Prestel contended for the Monthly Means as of practical value.

M. Bruhns read the opinions contained in the letters from MM. Dove, Mohn, Hoffmeyer, and Wolf. He himself voted for Daily, Five-day, and Monthly Means. Annual Means should be made according to Civil Reckoning. He himself always calculated two Annual Means, one for the Meteorological year, beginning with December, and the other for the Civil year.

Mr. Buchan remarked that he only wished to refer to the publication of annual results, and not the discussion of temperature observations. When only the means of observations for Five-days were published, many investigations, for which daily data were required, were rendered impossible. If, however, the daily results were published, all the data were afforded which could be required for any discussion of the observations. On this account he must declare himself in favour of Daily Means, and further, for Monthly and Annual Means according to the Civil Calendar. On the contrary he put no value on Seasonal means, as they were very delusive and easily led to quite false conclusions.

Mr. Scott gave Five-day Means, and also for the requirements of Civil life, Monthly and Yearly Means, according to the Civil Year.

M. von Oettingen spoke in favour of months composed of 5-day periods, and thought that the system recommended by M. Bruhns would be a great burden. He would combine every six 5-day intervals, beginning with December 2, to form a month, and have the one interval in excess embodied in November.

The Conference proceeded to division on three questions, whether Daily, Five-daily, and Monthly Means (for the Civil Month) should be laid down as a basis for meteorological calculations. The majority of the meeting decided for the division of the year into Days, Five-days, and Civil Months, as well as for the Civil Year.

They then proceeded further to discuss the question whether the year should be divided into 73 intervals of 5-days with M. Dove, or into 72 with M. Buys Ballot. Five-day means. Dove and Buys Ballot.

M. Buys Ballot said that he would rather not have joined in the discussion; even in his "Suggestions," in which all the questions which had been discussed hitherto had been mentioned, he had purposely omitted any allusion to the Five-day question, but he must express his preference for 72 intervals in order to obtain an agreement with the civil year. Up to the 31st May his

Calculation of means.

division agreed entirely with M. Dove's, after that date there was a difference, but the middle epochs of his Five-day (or Six-day) periods agreed, to within a fraction of a day.

M. von Oettingen was against the introduction of the Civil Year, but as the majority had expressed themselves in favour of it, it would be necessary to reject M. Dove's system. In this case he agreed with M. Buys Ballot, for if you once leave the natural division, you ought at least to make the artificial division as convenient as possible.

M. Prestel spoke for Dove's Five-day means because they had been already adopted and had been made the basis for such great investigations.

Mr. Scott, the President, proceeded to divide the meeting. 11 of those present were for Buys Ballot's intervals, 13 for Dove's. Messrs. Buchan and Buys Ballot declined to vote.

The question was briefly raised in what way the Annual Means should be obtained.

M. Bruhns obtains annual means by dividing the sum of the Daily Means by 365, Mr. Scott by dividing the Monthly Means by 12.

Sub-Committees for remaining questions.

M. Bruhns proposed that in order to bring into discussion at the next sitting, which was the last, the questions which had not yet been touched, two committees should be chosen, one for Questions 21–26, the other for Questions 7–17. The members of these committees should consider the various questions at a private meeting and submit definite proposals on them to the next meeting of the Conference.

This proposal was accepted, and MM. Bruhns, Jelinek, and Wild were chosen as members of the first Committee. For the second MM. Ebermayer, Hann, Prestel and Schoder.

The next and last meeting was fixed for the next day at noon. M. Wild was proposed as President, and the meeting was closed at 4 o'clock.

3. Protocol of the Sitting of the 16th of August.

M. Wild, as President, opened the meeting. He distributed a number of copies of M. Buys Ballot's paper, "Suggestions on a Uniform System, &c." and then called on the reporters of the two committees to present their reports to the meeting for consideration.

M. Jelinek as reporter of the first committee named at the last meeting presented his report on Questions 21–26.

Calculation of normal values.

21. In what manner and for what intervals are the Normal Values for the individual Meteorological Elements to be deduced?

One portion of this question has already received its answer in the result of the discussions on Question 20. It generally has to do with the calculation of shorter series of observations, and the committee therefore proposed a reduction to a normal

period, which might be a multiple of five years. A new period of this nature should begin with the 1st January 1871. Calculation of normal values.

M. von Oettingen proposed to adopt the name *lustral* mean for the 5-yearly mean.

Messrs. Bruhns and Scott expressed their agreement to these proposals, which were accepted by the meeting.

22. Is it desirable and possible to publish the Meteorological Observations of a limited number of Stations in each Country in a uniform manner, and within a reasonably short time after the Observations have been made? Uniformity of publication.

On this head the committee recommended the publication of original observations of a certain number of stations of each country, and proposed a scheme for the form of this publication which agreed very closely with the Russian system. Three observations a day should be given, of Pressure, Temperature, Relative Humidity, Wind Direction and Force, and Rainfall, and on a uniform system, so that the corresponding data of different stations and different countries could be immediately compared with each other. For the means, and whatever else should be added to such a publication, according to its character, (as, for instance, History of Weather and Special Phenomena), &c. there should be given a special place in the publication.

M. von Oettingen asked whether, when arranging this plan, regard had been had for the 5-day periods, so that separating spaces should be inserted between every five and five days; and whether, according to the proposal of the sub-committee, the Daily Means should be omitted.

M. Wild answered that on the first point there was no difficulty in the way of the arrangement proposed, and as regards the Daily Means they had only been left out of the proposal for want of room; but place might be found for them among the Means in the way which had been indicated.

Mr. Scott asked who was to print these publications, who was to pay the cost of them, and how many stations were to be included. A few stations would not be sufficient, and it would not be possible to put in all.

M. Bruhns explained the views of the committee. Each country was to bear the cost of its own publications, and also was entrusted with the choice of its stations according to its means. No one thought of a publication like an enlarged "Bulletin International." The proposed plan represented the minimum of requirements which it seemed necessary to propose with reference to the publication of original observations.

M. Sohnke spoke against the mode of publication which had been proposed, for he thought the observers whose results were not published would be impatient, and would give up their observations.

M. Jelinek explained the views of the committee, that it was only as regards the original observations that the restriction to a

Uniformity of publication.

limited number of stations should be introduced, but that the results—the means—should be inserted from *all* stations of the system.

M. Ragona asked where there was room for the daily Maxima and Minima in the plan proposed?

M. Bruhns answered that the plan was only a proposal and could be altered on certain heads.

Mr. Scott recommended that you should not pledge yourself to too much. He knew what the cost of publication of the automatic records of the seven stations of the Meteorological Committee cost. He thought that it would be unnecessary to give in addition to them the readings, for the curves were sufficient to give all the data required for every epoch.

M. von Oettingen denied this; he wished to put the greatest weight on the demand for obtaining the original observations, for no matter how good Mr. Scott's curves were for all investigations for which only single readings were wanted, the labour was incalculable and impracticable if certain mean values had to be deduced, when you had first to reduce all the numerous data. He recommended that proposals should be laid before the next Congress of various schemes in print, so that the members might be better able to decide on the merits of the respective plans.

M. Jelinek declared that it was the object of the committee to leave the decision of the question to the Congress. There were certain other questions which had not been touched. For instance, whether you should give the 5-day Means of the different stations of a country, together, or under each station. He replied with reference to Mr. Scott's objections, that if the proposal were taken up by the Congress in the way in which the committee proposed, it would be England which would have least to change. The object was twofold; firstly, to urge the countries, from which as yet no original observations have been obtainable, to publish such, and to introduce a certain conformity into the publications which already existed, in order to render comparisons and investigations of all sorts more easy.

M. Buys Ballot approved the plan and remarked only that Vapour Tension was omitted from it, information of which was desired by many investigators. Relative Humidity was less important than Vapour Tension, he preferred Absolute Humidity. He could not agree with the proposal of Yearly publications, the observations should be published Monthly, as was already done by Switzerland, Italy, Holland, Norway, &c.

M. Wild saw no great advantage in Monthly publications; he was decidedly in favour of Annual. Russia was not in a position to publish Monthly. There was, moreover, special importance attached to the question of these publications being as correct as possible. This was not attainable if you had to proceed to the publication of the observations immediately at the end of each month. There would always be then a long list of subsequent corrections. The Daily Weather Telegrams fully met the requirements of the study of recent weather phenomena. These

should certainly be published as quickly as possible. It was therefore desirable to recommend two kinds of publication, Yearly volumes and Meteorological Bulletins, the latter to be published at as short intervals as possible. Uniformity of publication.

M. Buys Ballot, notwithstanding M. Wild's representations, was in favour of Monthly publications, and this form might be accepted even for Russia if only a limited number of stations were adopted. The urgent requirement was to obtain a general view of the weather all over the globe at a time when the memory of the weather which had been experienced was still fresh.

Mr. Scott spoke against Monthly publications. Even in England they could not be carried out without difficulties, because the results of each month were not received at the Central Office until after the middle of the subsequent month.

M. Bruhns remarked finally that Vapour Tension had been only omitted from the scheme for want of room.

M. Ragona again urged the importance of the adoption of the daily Maxima and Minima in the publications.

The proposal of the committee to prepare plans for the next Congress was adopted.

23. How is the exchange of the Meteorological Publications of different Institutions and different Countries to be carried out most speedily, safely, and simply? Exchange of publications.

M. Jelinek reported for the committee, that parcels which were not too large might be most quickly and easily sent by book post. The distribution of reports to the individual observers of the meteorological systems could finally be carried out from the Central Offices. It was not advantageous to send through the Embassies on account of the delay and inconvenience; but in certain cases the Governments should be requested to instruct their Consulates to facilitate the intercourse with Foreign Institutes and Observatories, and to undertake the transmission of the publications. Another question was how acknowledgments for parcels received should be sent most cheaply and safely.

Several proposals were made in reference to this, especially the announcement of the forwarding of parcels by post-cards, with a coupon, which might be transmitted with the stamp of the receiving Institution as soon as the parcels had arrived safely.

M. Bruhns recommended that wherever there was a society as at Washington, Haarlem, &c. which offered to distribute publications in their own country use should be made of it; and that the Central Offices should designate certain institutes or firms who would undertake the transmission of publications for exchange.

24. Is it desirable that in each country there should be established one or more Central Institutions for the direction, collection, and publication of the Meteorological Observations?

M. Jelinek gave the report of the committee. The committee was of opinion that a Central Meteorological Institute

Establishment of central offices.

should be established in every large country for the objects named. For the purpose of uniform management it was desirable that there should be only *one* such central institute, and not several, in each country. Wherever, as *e.g.*, in Russia, the great extent of country makes it appear desirable, there might be Normal Observatories for certain regions which should undertake the superintendence and direction of stations situated near them. But these Normal Observatories should be always subordinate to the Central Meteorological Institute and should work according to the plan laid down by it. With regard to Marine Meteorological Observations, the committee expressed its opinion that there should be either an entirely separate office for Marine Observations, or that in the Meteorological Central Office there should be a separate Department instituted, with a separate head.

Mr. BUCHAN explained that the Scottish Meteorological Society was not in a position to agree to a proposal according to which there should be only *one* Central Office in Great Britain for the Management, Collection, and Publication of Meteorological Observations. The operations of that Society were conducted entirely independently of Government support. In fact most of the observers were not paid* for their observations. If a central office were established the result would be that the work, which was now done gratis, must be paid for. The hundreds of people who must then be employed would not work for nothing. He therefore thought that the question could not now be decided.

Mr. SCOTT said that several organizations already existed in England, thus Greenwich was under the Admiralty, while the seven normal observatories lately established were managed by the Meteorological Committee of the Royal Society.

The Conference adopted the resolution by 14 votes against 6, that—

"It is desirable that in each country One, or in case of "necessity Several, Central Offices should be established, and that "those for Sea and Land Observations should be separate."

Weather telegraphy.

25. Does the interchange of Weather Telegrams appear so useful that it should receive a fuller development and a firmer organization?

M. JELINEK reported for the committee that it was desirable that the present system of Weather Telegrams, which has already been established should receive further development.

Upon this the remarks on Weather Telegraphy contained in the report of the marine sub-committee were read (see p. 44.)

Mr. SCOTT recommended that the wish should be expressed that in every large country a central office should be established for Weather Telegraphy, as was already the case in most countries. A second question, in respect to the extension of the system of the

* Correction in English edition. For "most of the observers were not paid," read "none of the observers were paid."—A. BUCHAN.

stations which transmit Weather Reports to the Central Office of each country, and to the combination of foreign stations therewith, was explained by Mr. Scott, with reference to England, in the following sense: the principle adopted there was to pay attention only to the weather of the country itself, and for this purpose information from certain stations in neighbouring countries was indispensable; but it was not possible to meet other requirements of practical life, and *e.g.*, for Shipowners, to give Weather Telegrams from distant ports, which have nothing whatever to do with storm signals for the country to which they are sent. Weather telegraphy.

M. Wild said the main object of the committee's report was to influence Governments and the Telegraphic Directions.

M. Prestel recommended the extension of Telegraphic Weather Reports to the whole of Europe, and on the following plan:— a certain number of stations should be chosen, which should be just sufficient to give a general view of the changes and the conditions of the weather, and the telegrams of each of these stations should be sent to each of the others.

M. Bruhns said that the realization of this proposal would only be possible by the help of all the Governments.

M. von Oettingen said he received Daily Weather Telegrams, and could only lament that Germany took no part in the system, and that the form of the Weather Telegrams from Germany was defective. The communication of the absolute readings of meteorological instruments was certainly very desirable, but it was more urgent to give the Deviations from the Normal Values. It was only these deviations which gave intercomparable figures, and offered an immediate view of the conditions of the weather. It was not very useful to add the changes, in fact they might lead to very false results. Therefore he was decidedly disposed to support the communication of the Deviations from the normal values.

M. von Freeden thought that it would be often impossible to obtain the necessary Normal Values. He was also of opinion that the German Weather Telegrams had been formerly not quite satisfactory, but an improvement had been made in this respect. He recommended an arrangement of the stations in a N.E. and S.W. direction, as the best for the whole of Europe.

M. Bruhns proposed to refer the report of Weather Telegraphy for the Congress of 1873 to a sub-committee.

M. Wild thought that this matter might be handed over to the Marine sub-committee.

Mr. Scott was of the opinion that M. Prestel's proposal to send all telegrams to all stations was impracticable, and would break down on the point of the wordiness of the body of telegrams, which must cause confusion.

M. Wild said that for each country there was a definite area, within which those stations lie whose Weather Telegrams would be of immediate interest to the same, and this might influence the choice of the stations.

Upon this, Question 25 was handed over to the Marine sub-committee to furnish a report for the Congress.

Plans for Congress.

26. What Regulations should be adopted in order to carry into effect the Decisions and the Objects of the Meteorological Congress?

It was recommended to draw up a full Protocol of the Proceedings of this Preliminary Conference, and to establish a Permanent Bureau to prepare for the actual Congress next year.

M. Bruhns stated the reasons which were in favour of Vienna as the place for the Congress of 1873. They are the Exhibition, which would ensure the attendance of a large number of visitors, and the newly erected Meteorological Institute, whose complete equipment with new instruments might be expected by that time.

The proposal, then, to summon the Congress for next year for Vienna was unanimously adopted.

Similarly, on the motion of M. Buys Ballot, MM. Wild, Jelinek, and Bruhns were chosen unanimously for the Permanent Bureau.

M. Bruhns proposed to leave the duty of fixing the time for the invitation to the Meteorological Congress in 1873 to this Committee, and only in general terms to fix it for the end of September or the beginning of October.

The meeting agreed thereto.

M. von Oettingen proposed to establish an Organ for all preliminary arrangements which were to be made previous to the meeting of the Congress.

M. Wild was of opinion that the materials referred to would not be sufficient either in quantity, or in regularity of appearance, to require a special organ, but proposed to employ the Journal of the Austrian Meteorological Society as the organ.

M. Jelinek agreed, and promised to introduce all proceedings or communications, &c., which had reference to the Congress, into the Meteorological Journal or as an Appendix thereto.

The President, M. Wild, requested the second sub-committee, which had been appointed to discuss Questions 7 to 17, to submit their proposals to the meeting.

M. Hann as reporter for the committee, which consisted of MM. Ebermayer, Prestel, Schoder and himself, stated that they had arrived at the following conclusions.

Hygrometry.

7. What apparatus should be employed for the determination of the Hygrometric condition of the air? Are the Wet and Dry Bulb Thermometers sufficient? Can the Hair Hygrometer be employed, and under what restrictions?

The committee recommend that, inasmuch as the Psychrometer is untrustworthy at low temperatures, and also in cases of extreme dryness, and the Hair Hygrometer fails also at the dew point, and as up to this time there is no Hygrometer in existence,

at least for regular observations at stations, which gives precise results as to Moisture, in all cases; a wish should be expressed that there should be further experiments on the subject, and that the general experience on this question should be collected. Hygrometry.

M. Neumayer recommended the adoption of Regnault's Hygrometer with an aspirator. In Australia he had observed, every three hours, a Danicll's hygrometer with an aspirator, by the side of the Psychrometer, and had obtained thoroughly satisfactory results. You could hardly employ a more accurate instrument than that which he named for the systematic determination of the moisture of the air. It seemed to have been altogether forgotten, one of the reasons of which was certainly that Daniell's instrument in its original construction was not easily managed, but he himself, in very high temperatures, and extreme dryness, with 8% to 10% of saturation, had found no difficulty. However, in great heat, which comes in Australia as a result of the Northerly winds in summer, the air for the aspirator must be drawn from cool places.

M. von Oettingen remarked that with Regnault's Hygrometer he had not been able to obtain any deposit with very low temperatures, and that probably it would prove not practicable.

Mr. Scott described briefly the new Sulphuric Acid Hygrometer by Whitehouse, which perhaps might be generally employed.

M. Wild had for several years introduced Hair Hygrometers, in addition to Psychrometers, as an experiment, owing to the fact that at the majority of the Russian stations the Psychrometer is of no use in winter owing to the lowness of the temperature. According to the experience which had been already gained, they performed admirably, with few exceptions. If properly managed they could entirely replace the Psychrometer at 0° C. and at low temperatures, if they were occasionally verified by comparison with it above 0° C. He had moreover often found the two instruments in perfect accordance between − 5° and − 15°.

M. Neumayer remarked that Erman in his great travels round the world thought that he had gained the experience that the hair-hygrometer gave very good results, so that it would be well worth the trouble to take into consideration the use of the hair-hygrometer under certain conditions.

M. Bruhns said that the vapour tension of the air, at temperatures below zero, was sufficiently important to justify the proposal that experiments should be made with the Hair-hygrometer and the Psychrometer.

MM. Wild, Bruhns, and Jelinek expressed themselves ready to institute comparative observations and experiments on this subject.

M. Wild remarked that comparisons when the air was excessively dry were also very desirable, and he requested M. von Sternbach to make special observations on the occurrence of the Föhn-wind in winter at Bludenz.

Hygrometry.

M. von STERNBACH expressed his readiness to do so.

M. BUYS BALLOT undertook to make experiments with Baumhauer's Hygrometer.

Mr. SCOTT drew attention to the difficulties which were met in the employment of Self-registering Hygrometers, and recommended some comparisons with the Sulphuric-acid Hygrometer.

M. NEUMAYER drew the attention of the Conference to the importance of the investigation of the relations between Radiation and Moisture. A former difference of opinion on this important subject had now in great measure disappeared. An element which had such an influence on climatology could not be entirely disregarded, therefore he recommended to the sub-committee to consider Questions 5 to 7 specially.

Mr. BUCHAN agreed with the preceding speaker, that the question of the action of Radiation and of Latent Heat should be thoroughly investigated, and it was especially the duty of the great observatories to undertake the complete treatment of this question. Much which was as yet unintelligible in the climates of Australia and India would be explained by such enquiries. Whoever really works up the Hygrometrical question in its relation to Radiation will, if a few results only be obtained, make a great step in the advancement of science. He said that he had spoken strongly on this subject, but certainly not too much so.

M. PRESTEL expressed his opinion that simple relations might be established between the extent of Evaporation and the condition of Relative Humidity, and the subject should be specially investigated.

Direction of wind.

8a. In what way can uniformity in describing the Directions of the wind be attained?

The sub-committee recommended, in order to remove the difference between the nomenclature of the East and West winds in the Latin and Teutonic languages, and to avoid the misunderstanding which arises from the use of the letter "O," to introduce in general the letter "E" for abbreviation for East wind, "W" for West wind, *i.e.*, the designations used in England; a proposal which was received by the Conference after some remarks.

M. BUYS BALLOT recommended the use of arrows, in place of letters, for indicating the Direction of the Wind.

M. von OETTINGEN said that even if arrows were introduced, you could not avoid using letters in many cases; as, for instance, in the Telegraphic Weather Reports published in the newspapers. Otherwise he was for the introduction of the arrows, where it was only the facility of inspection which had to be considered. But for calculation you must introduce another mode of describing the wind. In this case the 8 or 16 directions were insufficient; you must come to the Components, as he had proved and maintained in the last volumes of the Dorpat Meteorological Observations.

8b. Is the deduction of the mean Direction of the Wind by Lambert's formula desirable? Is it desirable or not to include very light winds (force 0) in constructing Wind Roses for the Direction of the Wind? Lambert's formula.

The sub-committee stated their opinion that the use of Lambert's Formula was not advisable. The Calms should be introduced separately, and designated by a separate abbreviation, such as the letter "C."

M. von OETTINGEN spoke in favour of his mode of discussion of the wind. He explained briefly the construction of his new anemometer, which reduces the wind immediately, according to direction and force, to the four axes of a system of rectangular co-ordinates, and so meets all the requirements which could be made from a mechanical point of view, for the representation of the movements of the air. The description of the apparatus would be given at greater length in the next Dorpat Report. He hoped to be able to submit the apparatus itself to the Meteorological Congress at Vienna, and to give the results obtained by it at full length in a paper.

M. HANN spoke in favour of the recommendations of the sub-committee and against Lambert's Formula. As regards the latter in a form of reduction to a single resultant, which had originally been adopted, usually without attention to the Force, enough had been written already, and its worthlessness had been sufficiently proved. M. von Oettingen also declined to follow the last step in using it, and had not gone further than the four Components. But these were not at all sufficient for all investigations, especially of a Climatological nature. If you took the four Components, it was not very easy to see why you should not take eight Components, that is the usual eight principal points without any reduction.

There were two points to be decided and fixed. If the question was the communication of results from the observations, a reduction to four Components might be justifiable. But this was certainly not the case when you had to deal with the publication of original observations. In these you could not insert values which had been already reduced, and were only useful for one definite series of investigations, but must give the observations themselves, in order that they might be employed for investigations of any kind.

M. von Oettingen's proposal was perfectly right and satisfactory as long as the question of the Quantities of Air in motion was dealt with, but this was not the only question. In Climatological investigations, for instance, it was especially the Frequency of the different winds which had to be considered, without reference to their force, and the four components were insufficient for representing the wind conditions which were characteristic for the various climatological areas, and they in many cases actually led to wrong conclusions. In the original observations it was necessary therefore to give at least eight directions, and if you gave the

force as well, the reduction to four components, for the object which had first been named, could easily be carried out at any time.

The proposals of the sub-committee were adopted by 12 votes to 7.

Scale of wind force.

9. What scale should be employed for the Force of the Wind when it is not determined by actual measurement, but only by estimation?

Recommendation of sub-committee. The scale of 4 for Wind Force is insufficient, according to the experience which has been gained; that of 10, on the contrary, gives too minute distinctions, which allow too great latitude for choice when the wind force *is simply estimated*, and therefore the scale of 6 is to be recommended. It is already introduced in large systems of observations, and presents at the same time the advantage of easy reduction to the Beaufort's scale, which will soon come to be adopted generally at sea.

M. Jelinek spoke in favour of the scale of 10, which is now most generally employed, and seems best adapted to the metric scale and to other meteorological measurements.

M. von Sternbach also defended the scale of 10, which he thought must be called even insufficient for the more accurate determination of the force of wind.

M. von Oettingen said the first point was to give a precise definition of the scale of 6; without that he thought the proposal insufficient. For every figure of the scale the corresponding velocity must be assigned.

Mr. Scott said that the scale of 6 did not give minute enough distinctions. He explained the principle on which Beaufort's scale of wind force was based, and said it was very difficult to give the velocities which corresponded to each figure of the scale.

M. Bruhns spoke in favour of the scale of 10, and suggested that the determination of the question should be postponed until the next Congress.

M. Wild briefly described the experiments which had been made with simple apparatus for the determination of the Force of the wind, with respect to which he referred to the St. Petersburg bulletin.

M. Bruhns proposed that observations should be made at several observatories on the relation which the estimated Force bore to the measured Velocity, in order to give a basis for determining the scale.

M. Jelinek thought that such an investigation should be carried out as well at the observatories on the continent, where the scales of 10 or 6 are used, as at sea, or on the coast where the Beaufort scale of 12 is employed.

M. Wild remarked that the Velocity of the wind depended on the altitude at which the anemometer was erected; and its height above sea level must also be taken into consideration.

M. von FREEDEN drew attention to the circumstance that, when the anemometer has been erected on the flat roof of a tower, it has happened, in consequence of the ascensional movement of the air at the building when the storm blew straight against it, that the anemometer gave a velocity far too low, inasmuch as there was a calm on the actual roof. Scale of wind force.

Mr. SCOTT agreed with this remark from his own experience.

The Conference referred the question of the comparisons, and the determination of a scale of Wind Force, to Messrs. BUYS BALLOT, JELINEK, and SCOTT.

10. Is it desirable to introduce simple counting instruments for determining the Velocity of the Wind? What units should be taken for the discussion of the Velocity of the Wind? Velocity of wind.

The proposal of the sub-committee went so far as to recommend the adoption of Robinson's Anemometer in its simplest form for the determination of the Velocity of the wind prevailing at the time of observation. In agreement with the unit which is taken in all physical works, the velocities should be given in Metres per Second.

Mr. SCOTT remarked with reference to Robinson's anemometer that he hoped that soon experiments would be instituted in England in order to determine whether the simple relation between velocity and the number of revolutions of the cups was quite correct or not.

The recommendation of the sub-committee was adopted.

11. What is the best form, size, and mode of exposure of Rain gauges? At what hour of the day should the fall be measured? Rain gauges.

The recommendation of the committee went so far as to suggest that a complete report of the experience which had been as yet gained, with reference to the first portion of the question, should be prepared by a committee for the Congress of 1873. As for the hour of observation, an hour in the morning should be recommended, if the conditions permitted it, and, in addition, an hour in the evening.

Mr. BUCHAN remarked that the first portion of the question had been almost exhaustively settled by experiments and observations, accounts of which would be found in Symon's "British Rainfall" and the "Meteorological Magazine."

M. GALLE drew attention to the great influence of height on the erection of rain gauges, and drew attention to the great mass of observations which had been accumulated in Scotland from 1854 to the present time.

Mr. BRUHNS proposed that it should be recommended that all rain gauges should be of the same form, the same area, and erected at the same height; that the best receivers were circular, $\frac{1}{10}$th of a square metre in area, at a height of $2\frac{1}{2}$ metres abve the ground;

Rain gauges.

but it was still desirable that at the central stations, in addition to their present rain gauges, others should be erected, by which the dependence of the quantity of rain collected on the size and form of the rain gauge should be determined.

M. Buys Ballot supported the proposal and the meeting adopted it.

M. Buys Ballot remarked, with reference to the second point of Question 11, that he should like to reverse the recommendation of the committee, viz., that the original recommendation should be to measure the rain in the evening, and if possible in the morning also. By this means you avoid the error which arises if the rain which is measured in the morning is put down in the register for the day on which it is measured.

M. von Oettingen remarked that the answer to this question depended on the decision which was to be made as to the commencement of the meteorological day.

M. Wild, for the same reason, recommended that this question should be left quite open for the next congress and M. Jelinek agreed with him.

The meeting adopted the proposal of leaving the question open.

Rain and snow.

12. Should the days of Rain and Snow be counted separately or together?

The recommendation of the committee was that there should be three columns in the observing form, headed Rain, Snow, and Mixed Days (days with rain and snow.)

M. von Sternbach remarked that by this the duty of the observers would be much increased, for they must constantly be watching whether a shower of rain had not turned to snow, and the contrary.

M. Wild answered that each observer would only have to enter his observations as before, and that a completely accurate registration was impossible, inasmuch as observers could only put down in a general way what fell during the night.

The recommendation of the Committee was adopted.

Hail.

13. Is it desirable in giving the falls of Hail to draw a distinction between "Graupel" and true Hail?

The Committee recommended to carry out this separation, as far as possible, in the interests of agriculture and practical life.

M. Bruhns declared that he agreed with the proposal.

M. Sohnke opposed the proposal, for it was impossible to draw a strict line between "graupel" and hail; therefore the distinction could not be maintained, and was unscientific.

M. Wild remarked that according to the Russian instructions hail and "graupel" were separated. Hail was only given with thunderstorms.

M. PRESTEL said separation was very possible. Hail had always an envelope of clear ice; the grain of "graupel" could be clearly recognized as a spherical snow-flake. He drew finer distinctions, for "gries," "graupel," and hail. Hail.

14. In counting Thunderstorms, should the Storms as such or the Days of Storm be given? In what way should the instances of Sheet-lightning be noticed? Thunder-storms.

Recommendation of the Committee: Only Days of Storm should be counted, and not the separate storms. Sheet-lightning should also be given, if it be independent, and not a consequence of storm at the place of observation.

15. What apparatus is to be recommended for the measurement of Evaporation? What is the most suitable exposure for the Vaporimeter? Evaporation.

The sub-committee recommended the Conference to refer this question to a committee for a report for the Congress of 1873, as it still required an accurate investigation.

M. PRESTEL recommended his vaporimeter, and referred to the description in the Austrian Journal for Meteorology, Vol. I., p. 193.

M. EBERMAYER had made very good observations with Lamont's vaporimeter, and he therefore thought that he could recommend it; and his apparatus also for the determination of the evaporation from the soil, when kept moist, acted quite satisfactorily. With reference to this circumstance he referred to his work, "Die "physikalische Einwirkung des Waldes auf Luft und Boden."

M. NEUMAYER said that he had also made observations with various vaporimeters in Australia, but he thought that it would not be in the interest of the Conference to enter into these details of the description of instruments. On this subject, as well as on several others on which he had not expressed his opinion, for similar reasons, he would place himself in communication with the committee, and give them his experience; a proceeding which would be very much to be recommended, in the interest of the subject, to the other gentlemen.

16. In what way should the proportion of Cloud in the sky be estimated and indicated? Is it desirable to introduce for Clouds, Hydrometeors, and for other extraordinary phenomena, a nomenclature which shall be independent of local language, and therefore universally intelligible? Proportion of cloud.

The Committee recommended to give the degree of cloudiness of the sky by figures from 0 to 10. "0" should be "blue sky," and "10" "entirely overcast." The introduction of generally intelligible symbols for the different classes of hydrometeors

(except the proportion of Cloud, for which figures are proposed), as for Rain, Snow, Hail, "Graupel," Thunderstorms, &c., was desirable. A table of the symbols which are used at the present time in various systems of observations, should be submitted to the Congress, to make a choice from them.

Extra observations.

17. Should other Meteorological Elements than those already enumerated, *e.g.*, Atmospheric Electricity, &c., be included in the scope of normal observations, and what are the best instruments for observing them?

The recommendation of the sub-committee was to the effect that this question, which could only have reference to large observatories, should also be entrusted to a committee for the preparation of a report for the Congress.

M. PRESTEL reminded the Conference of the importance of Ozone observations, and the desirability of uniformity in the Ozone papers and scales employed for observations.

M. EBERMAYER recommended observations on the level of the water in the soil, and on the introduction of rain-water into the soil, by means of the Lysometer.

In consequence of the lateness of the hour, which forbad a thorough discussion of Questions 13–17, the Conference, on M. BRUHNS' proposal, requested MM. EBERMAYER, SCHODER and SOHNKE to give a report on the questions mentioned to the Congress of 1873.

Finally, at the request of the President, the report of the sub-committee on Marine Meteorology was submitted by M. NEUMAYER.

REPORT OF THE SUB-COMMITTEE ON MARINE METEOROLOGY TO THE METEOROLOGICAL CONFERENCE AT LEIPZIG.

Meeting of the 16th August, at 7h. 30m. a.m.

Present: MM. BUYS BALLOT (Chairman), BUCHAN, von FREEDEN, MÜLLER (Secretary), NEUMAYER, and SCOTT.

After reading the Resolution which had been adopted by the Conference at its meeting on the previous day, with respect to the nomination of the sub-committee, the five points proposed by M. NEUMAYER were read as a basis for discussion. The questions were handled at considerable length, and the discussion was extended to minute details.

Uniformity in methods and instruments.

1st. Thorough uniformity in methods and instruments should be aimed at, in the same measure as for observations on shore. This will be most satisfactorily attained by the chiefs of the central institutes—the establishment of which in all countries, in which they do not already exist, and in which the maritime in-

terests demand them, must be declared as absolutely necessary— entering into relations with each other and agreeing on the separate details, the construction of the instruments, the hours of observation, the journal, &c.

This proposal was unanimously adopted after a short discussion.

2nd. Unity of measures and scales is desirable, and to this end the introduction of millimètres for the barometer and the centigrade scale for the thermometer should be aimed at. While, however, the comparison of standard instruments of the individual central stations must be insisted on, the uniformity of scales is at present only declared as desirable. Unity of scales.

A long discussion ensued on this question, the result of which was that none of the gentlemen present undervalued the importance of a uniform measure, Metre and Celsius. But, for use at sea, they found that the maintenance of the English barometer scale by certain nations might still be justified, in consequence of the difficulty of obtaining uniformity on the point; they must however express themselves against the use of *two* distinct measures.

M. Buys Ballot expressed this opinion in this sense: that he was so convinced of the necessity of a uniform scale that he himself would give up millimètres, for the English measures, on the condition that uniformity could be obtained thereby.

3rd. The committee would urge the importance of the co-operation of the Navies, inasmuch as by their assistance, and by the opportunities afforded thereby of completeness in certain observations, the determination of factors and constants is rendered possible, which can be used with advantage for the reduction of certain results derived from the general system of observations. Co-operation of navies.

The sub-committee unanimously adopted this resolution also after a long discussion.

4th. With reference to the utilization of the results, the committee would urge similarly the importance of uniformity in the methods employed. In close relation therewith was the carrying out of the division of labour of the central stations of the individual states. This principle must be recognised as of the greatest importance for the further development of Marine Meteorology. The repetition of work over definite regions, with reference to the area to be investigated, must be declared as indefensible in the interests of this development. Division of work.

Mr. Scott remarked that the Meteorological Office in London had determined hitherto only to permit the copying of any scientific documents on the condition that the applicant paid the cost.

On the contrary M. Buys Ballot remarked that he, as formerly, would be prepared in future to adopt the consequences of the proposal which was submitted; and all who were present finally agreed thereto.

M. von Freeden observed that up to the present time the matter had been arranged by mutual agreement, and that a pressure could hardly be exerted.

Weather telegraphy.

5th. The sub-committee would recommend that the Conference should, to be in unison with most authorities of science, express itself to the effect that the question of the practicability and utility of Weather and Storm Signals in Europe must still be considered as an open one from many points of view. They would recognise the importance of more thoroughly sifting the material which is in existence, and for this purpose named a sub-committee which should consist of MM. Buys Ballot, Scott, and Neumayer, with power to increase their number. All authorities in this branch of science, among whom M. Mohn was especially named, should be requested to give their opinion, and the results of this investigation should be drawn up in a report which should be published at least two months before the meeting of the Congress in Vienna.

After some discussion with respect to the great difficulties which were connected with carrying out this proposal, the sub-committee passed unanimously the resolution to adopt this point also.

The sub-committee recommended the Meteorological Conference to adopt all these their conclusions, and in this case prayed it to embody the present Report in the general Protocols of the Conference.

Finally, after several questions and debates, the Conference adopted all the proposals of the sub-committee.

M. Jelinek, in the name of the issuers of the invitation, expressed to the Conference their heartiest thanks for the active co-operation they had met with at its hands, and at 4 o'clock the Conference of Meteorologists in Leipzig was declared closed.

APPENDIX.

APPENDIX A.

Communications addressed to the Meteorological Conference in Leipzig.

I.

Geh. Regierungsrath H. W. Dove to Director C. Bruhns, Leipzig.

Berlin, July 24th, 1872.

I beg you to excuse my attendance in Leipzig as I must get some mountain air in August. As for the questions to be laid before the Conference with the hope of arriving at some general agreement, I should wish that unanimity were attained on the following heads :—

1. *Division of Time.*—For Non-Periodic Variations the hours 6, 2, 10, seem to me the best arrangement, with Five-day means for the yearly period, beginning with January 1st. For the years and months I think it best to hold to the old division, as a unanimous agreement of all observers to a change is hardly probable; of course Old Style is inadmissible, we must use New Style. Uniform hours for the daily means for all countries are scarcely to be hoped for, as the hours convenient in one country are not so elsewhere. At all events it seems to me necessary to state what correction has been applied to reduce the mean of the hours of observation to a true mean. It seems to me very desirable to give, in addition to the means for the individual stations, the reduction to a normal station, as I have always done in the case of my stations. If no such normal station exists, observations at similar hours are the only ones we can employ in isothermal inquiries. Inasmuch as the hours which suit one instrument are not necessarily suitable for other instruments, it appears to me advisable to consider the Thermometer as the instrument which rules the choice of hours, owing to the magnitude of the diurnal variation of temperature.

By time I mean local time; this certainly interferes with the investigation of really simultaneous phenomena. To refer everything to the time of one place, as was done by Gauss in his inquiries on Terrestrial Magnetism, seems to me impracticable for Meteorological investigations, and rather disadvantageous than otherwise.

2. I have already proposed, and still think it desirable, to adopt the English denominations for the direction of the wind, E. for east and W. for west, for then there is no possibility of error for Germans and Frenchmen. The plan employed in the Swiss system seems to me thoroughly unpractical, where we have, in the same book, different terms for the stations where French or German are spoken respectively. The employment of 8 or 16 points for the wind may be left to the observer if the vane is not graduated so as to give degrees. If there be degrees, however, it must be settled what degrees correspond to the several points, *e.g.* N.N.E. from to . The degrees should be counted as on a clock face, and also counted in the same way in the Northern and Southern Hemispheres, even though the direction of gyration of the wind is different.

I think Robinson's principle the best for anemometrical inquiries. The vane must possess an absolutely free exposure. The determination of the distribution of Pressure, Temperature, and Vapour in the windrose is desirable, together with the law of variations of the instruments, which has only been ascertained for a few stations, by comparing the observation before the shift of the wind to a certain point with that after the shift.

3. *The Scale.*—The introduction of the Centigrade scale into meteorological inquiries does not appear advisable to me until the English and Americans give up their Fahrenheit's scale. The division to tenths is universally referred

to the unit, whatever the degree is. In all the older investigations Fahrenheit and Reaumur's scales are so decidedly predominant that I have always preferred Reaumur's scale for my inquiries. The attempt of Forbes at Chiswick to introduce the Centigrade scale instead of Fahrenheit's failed completely. Fahrenheit's scale offers the advantage of giving only positive quantities almost all over the Temperate Zone. Negative degrees could certainly be avoided, if a new scale were introduced, with 100 added to it. This addition could be made to the older observations. The sole inconvenience in the Fahrenheit scale is found in inquiries on the distribution of plants, as we then only use readings above the freezing point.

The introduction of the Metrical scale for Barometers meets with a difficulty in the fact that maritime nations use the English scale almost universally. In this the unit is the inch, not, as in the Russian system, the half inch.

The remarks on the Barometer apply also to Rain, which I am convinced should be given, not in the cubic quantity of rain which falls, referred to a definite unit of area, but in depth. It is absolutely necessary to reduce, by melting it, the amount collected in a solid form to the liquid state. It seems to me, however, desirable to make a distinction between snow and rain in giving the quantity of rain.

4. For *storm signals* the adoption of a common system appears to me absolutely necessary, so that the sailor may expect the same language in all parts.

Whether only daily means or the observations of individual hours should be published, must be so entirely decided by the amount of means at disposal that no general arrangements can be made on the subject. The sole requirement which must be complied with is to calculate observations of the several hours to daily (five-day) and monthly means, as it is not fair to have to carry out these calculations when a journal comes to be discussed.

5. *Moisture* is to be given relatively and absolutely, *i.e.*, as per-centages of Saturation and by Tension of Vapour.

II.

Dr. J. Estreber, Director of the Statistical Department, Costa Rica.

Berlin, August 14th, 1872.

I see from the newspapers that, on the occasion of the meeting of the Naturforscher Versammlung in Leipzig, between the 12th and 18th, a conference is to be held, preliminary to the proposed International Congress, the object of which is the concentration of Meteorology.

The undersigned, who is Director of the Statistical Department in the republic of Costa Rica, in Central America, is at present resident in this country, being attached to an embassy from that state to Germany. On this account it is not possible for him to take part in the conference as he would have wished. In the progress of his statistical studies the subject of meteorology is constantly and fully brought before his notice, and he endeavours to prosecute inquiries in that science to the utmost extent that the resources of that small and young state allow him. He therefore thinks that it may not be unacceptable if he offers for the future to contribute his mite to the scientific results of the Congress.

The Isthmus of Costa Rica, across which an inter-oceanic railway is in process of construction, and where the preliminaries for the construction of a ship canal are in progress, is a region specially interesting and fruitful for meteorology, and there can be no doubt that the results of the Congress will tend to assist my modest efforts, and to direct them in the right path.

In the above sense I venture to take part in your proceedings by letter, and in my own name and that of my Government beg for the honour of a reply to inform me whether this my promise of co-operation can be recognized or not.

III.

Sr. Fradesso da Silveira, Director of the Meteorological and Magnetical Observatory of the Infante Don Luiz, Lisbon, 5th August 1872.

Having received the invitation to attend the Meteorological Conference at Leipzig, which you have been good enough to send me, and being unable to take part in the meeting, I have the honour to forward you my opinion, and that of MM. de Brito-Capello and Gama Lobo, the heads of departments here, with respect to the questions which you wish to see placed on the programme of the International Congress to be held at Vienna in the autumn of 1873.

I shall follow your questions in detail:—

1. Doubtless. The Centigrade scale should be used for thermometers, the Metric scale for the barometer, the velocity of the wind, &c. But if this cannot be effected at once, at least the mean results, and the maxima and minima, should be referred to those scales in all publications. Uniformity of scales, p. 11.

2. Adie's (mercurial) barometer seems to us the most suitable. We adopt it for ships, and for the meteorological stations affiliated to the observatory, and only employ the aneroid to replace it temporarily, or for making comparisons at the moment of reading. Barometers, p. 13.

Stanley's aneroid might perhaps be suitable, but we are not yet sufficiently acquainted with it to venture a conscientious opinion about it.

3. They should be exposed to the free action of the wind from every direction, sheltered from the sun, rain, and radiation, and protected by double louvred screens. Exposure of thermometers, p. 14.

4. We prefer Negretti and Zambra's maximum, and Rutherford's (spirit) minimum. Maximum and minimum thermometers, p. 17.

5. The black-bulb maximum thermometer, in vacuo, for solar radiation, and for nocturnal radiation, the black-bulb minimum, in vacuo, placed in the focus of a parabolic mirror. Radiation, p. 19.

6. We cannot reply to this question from our own experience. Earth temperature, p. 19.

7. For a continuous series of observations we prefer the psychrometer. It seems to us that the hair-hygrometer, although improved, can never give precise indications in open air. Hygrometry, p. 34.

8. The winds should be denoted by the 16 rhumbs of 22° 30′ each, and the same figures or letters should be universally adopted. Direction of wind, pp. 36, 37.

In observatories of the first order, where the velocities are determined, Lambert's formula may be used for obtaining the mean direction, if the product of the winds' frequency by the corresponding velocities be substituted for the numbers indicating the frequency.

At stations of the second order the same formula may also be employed, if the product of the numbers which represent the frequency by those which represent the corresponding mean forces be introduced.

I think the error would be less if two forces denoted by zero, and of the same direction, were counted as one unit, than if they were neglected altogether.

9. A numerical scale, provided that the same scale is everywhere adopted. Scale of wind force, p. 38.

At stations of the second order we employ 8 degrees of force, denoted by the figures 0 to 7. (See our Report for 1871, p. 70.*)

10. The adoption of counting instruments would be very convenient. A simple and cheap anemometer should be used, to register the maximum force of wind in a given time. If this were done in every meteorological institute, the force of wind should be noted at the time of each observation at stations of the second order, and it should be represented by the well-known terms, "strong," "moderate," &c., or better still, by the figures of a generally adopted table. Velocity of wind, p. 39.

At the stations of the second order in Portugal we employ Robinson's anemometer, the scale being adapted for giving kilometres.

11. At the observatory we have the self-registering pluviometer, and one of Babinet's rain gauges at each station. We also use this rain gauge at the central observatory. Rain gauges p. 39.

We prefer the hour of 9 a.m. for reading the instrument.

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* Report for 1872, p. 70.

Separation of rain from snow, p. 40.

12. Doubtless. It is only at some of our stations of the second order that we have to count the days of snow.

"Graupel" from hail, p. 40.

13. If possible. Too many details would perhaps be prejudicial, especially at stations of the second order.

Thunderstorms, p. 41.

14. The number of thunderstorms, and the number of days, should be separately counted.

Heat-lightning is always noted in the registers of the observatory.

Evaporation, p. 41.

15. Whatever vaporimeter is adopted, the same pattern must be established for all observatories and for all their stations.

It seems to us that it should be in the open air, exposed to rain, sun, &c.

The description of the form we have adopted will be found at p. 60 of our Report for 1871.

Proportion of cloud, p. 41.

16. In our opinion the convention of tenths of clear sky should be adopted for the amount of cloud. Thus 10 would signify clear sky, and 0 overcast. In any case a general convention is wanted.

It seems to us that the introduction of universal symbols would give great facilities. For instance, those found in the tables of Montsouris Observatory might be adopted.

Atmospheric electricity, p. 42.

17. Doubtless, in establishments of the first order. We have tried the continuously registering Electrometer. But unfortunately this instrument is not sufficiently perfected to merit complete confidence.

It is to be hoped, however, that an electrograph will give considerable results to science. We are inclined to believe it will.

Uniform times of observation, p. 20.

18. This is very desirable, but not easy at stations of the second order, where the staff is badly paid, or is not paid at all. The duties at such stations should be amalgamated with others, with schools, telegraphs, lighthouses, &c., in order to get a good organization. Then certain rules could be laid down, and their observance demanded.

Inspections and verifications, p. 23.

19. Certainly, but always in accordance with our reply to the 18th question.

Calculation of means, p. 25.

20. We think that observatories of the first order should calculate the means of the following elements daily, viz., Pressure, Temperature, Humidity, Velocity, and approximate Mean Direction of the Wind. For these and other elements means for decades, months, seasons, and the year should be calculated, noting the corresponding maxima and minima.

Mean values and resumés might also be calculated for five-day periods.

For the sake of uniformity we prefer to reckon the meteorological year from the 1st January. We actually do reckon it from the 1st of December.

Calculation of normal values, p. 28.

21. They must be deduced from the greatest possible number of observations, and since disturbances and the accidental variations of the different elements cannot be eliminated, we must determine the normal values from all these observations.

Uniformity of publications, p. 29.

22. Doubtless; but a common plan must be adopted for the publication.

Exchange of publications, p. 31.

23. By being transmitted post free, for the cost at present does not admit of a general exchange. If free transport is authorised by an international convention, three deliverics per month might be made.

Establishment of central offices, p. 31.

24. This service ought to be centralized, first in an observatory of the first order in each country, and then, if practicable, there ought to be an establishment which centralizes the works of different countries, to publish their general results.

Weather telegraphy, p. 32.

25. It appears to us that the exchange of meteorological telegrams becomes indispensable; but this interchange will not produce the results which might be expected, until all countries take suitable steps for preventing delay in the transmission of bulletins.

We often do not receive the Paris telegrams in time for the calculation of the probable weather for the morrow, which we publish daily in the official journal and in many other papers.

In the concession for the line from Lisbon to Brazil, by Madeira and St. Vincent, the Government has taken the requirements of meteorology into

consideration, and has inserted in the concession our right to two telegrams per day, gratis, between these two points and Lisbon. Weather telegraphy.

We count upon similar conditions in every new concession. If, for example, a line is laid to the Azores, we shall have free meteorological telegrams. This advantage will enable us to render great services to meteorology, if Dr. Buys Ballot's plan is realized, as we think it will be, by the establishment of an important station on one of the islands of Corvo or Flores.

26. Those who conceived the happy idea of initiating the convocation of a preparatory meeting and the *savans* who will assist at the Vienna Congress will no doubt be able to obtain an International Convention to ensure the fulfilment of the decisions of the Congress. It is necessary to do so, for without that all our labours would end simply in a manifestation of our desires. Plans for Congress, p. 34.

The observatory of the Infante Don Luiz, for reasons which I need not discuss here, will probably have no representative at the Congress of Vienna.

If so, and I do not doubt it, I shall hasten to profit by the results of the Conference at Leipzig, to set forth the propositions which I consider indispensable to the success of an enterprise with which so many interests are allied.

Your set of questions, the publication of "Suggestions on a uniform System of Meteorological Observations," by Dr. Buys Ballot, Director of the Royal Meteorological Institute of the Netherlands, and the works of the Observatory of Paris, and several other establishments, for the Congress of Vienna, make it our duty to contribute as much as possible by placing at the disposal of the Congress all the elements which it may require and which depend on the special position of the Observatory of Lisbon.

IV.

M. Carl Fritsch, late Vice-director of the K.K. Central Anstalt für Meteorologie.

Salzburg, July 26th, 1872.

The numbers refer to the questions.

1. The introduction of similar units of scale for all meteorological measurements in all countries is certainly very desirable. Inasmuch, however, as such a general introduction will hardly ever be carried out, it is sufficient, or rather, it is unavoidable, to settle definite rules for the reduction of the measures in use. Uniformity of scales, p. 11.

2. The most suitable construction of barometers for stations of the second order is, in general, that which will admit of as rapid and certain a mode of determining the height of the column as is possible, without paying attention to extremely minute accuracy. Barometers, p. 13.

All reductions should be confined to that for the temperature of the mercury. A syphon barometer with rack work and a spring to move the scale, as at the Saxon stations, but without a microscope, is quite sufficient. I myself have formerly employed a similar instrument for years to my complete satisfaction. At the observatory organized by Kreil, at Prague, in 1839, the observations were at first made with this instrument. The use of aneroids at such stations cannot be sanctioned.

3. The best mode of exposure of thermometers for the determination of air temperature is in general to place them quite in the open air, and at a slight height, about two metres, above the ground. A cord or wire should be stretched between two posts, erected at a proper distance from each other, and the thermometer should hang from the centre of the wire. Exposure of thermometers, p. 14.

It is unavoidable, with such an exposure, that the thermometer should be exposed to rain and sunshine. If, however, the bulb of a thermometer graduated on glass is in the form of a cylinder of small diameter, the effect, either of evaporation of rain water, or of insolation, is slight, and, moreover, compensates itself, at least in some measure. At all events the disturbing influences, with such an exposure, would be less than with any other near a building, whether sheltered by a screen or not.

Such a mode of exposure should be at least introduced at all central observatories, which are almost always in a position to provide the necessary sites,

even though it cannot be recommended for general adoption in respect of the last-named condition. I can cite Lamont to confirm my views.*

Maximum and minimum thermometers, p. 17.

4. Walferdin's maximum thermometer, and Baudin's minimum thermometer, seem to me the best to be recommended for general use in determining the absolute extremes of temperature.

Earth temperatures, p. 19.

6. For the determination of earth temperatures, I prefer the arrangement which admits of the use of thermometers of ordinary dimensions, which can, therefore, be sunk into the soil at pleasure and withdrawn again. The apparatus used by Lamont appears to me quite suitable.

The successive depths at which the temperature is determined should extend to the stratum of constant temperature, and may be increased in a certain proportion according as that is approached.

Hygrometry, p. 34.

7. In my opinion the psychrometer is sufficient for the determination of the hygrometric state of the atmosphere, and is certainly preferable to the hair hygrometer.

I consider that the keeping the wet bulb constantly wet, or rather the confidence of the observer on that point, which is usually excessive, is the chief source of error. I therefore recommend wetting the bulb before each observation, as is at all events a matter of course in winter, about a quarter or half an hour before each observation.

Direction of wind, pp. 36, 37.

8. On this head I refer to my remarks, on § 1. It might be well to limit the directions to the eight principal points, N., N.E., E., S.E., &c. The deduction of the mean direction of wind by Lambert's formula seems to me desirable in all cases where we have to deal with summarized results and slight changes in direction of the wind. Its employment in the representation of the diurnal period of the wind's direction would be of great use.

The very light winds should also be regarded in the distribution of the directions in the windrose, as a definite limit can hardly be given for the force.

Scale of wind force, p. 38.

9. Where a simple estimation of the force of the wind is given, a scale with few divisions is sufficient, and this is therefore all that needs confirmation. I am, however, for the retention of the scale of 10, because it has been used generally and for a long time.

Velocity of wind, p. 39.

10. I hold the introduction of a simple counting apparatus for determining the velocity of the wind as very desirable, and I should be still more pleased if it could entirely replace the arbitrary, and therefore hardly intercomparable, estimation of the force (Question 9).

The distance in metres traversed by the wind in a second might be taken as the unit for velocity.

Rain gauges, p. 39.

11. The rain gauges used by the Central Anstalt in Vienna seem to me quite suitable. I should only recommend that the receiving vessels should be provided with handles and that two receiving vessels, or still better, two complete gauges, should be issued to each station, which could be interchanged on days of fall, or also in the interval between two measurements, with unusually heavy or continuous falls, which would cause anxiety lest the gauge should not be large enough to hold the entire quantity which has fallen. This is not an uncommon occurrence with snow, and it also not unfrequently happens that a part of the fall remains in the receiving vessel, in the form of snow or hail, while the remainder is in the collecting vessel, in the form of water which arises from a thaw, or from rain which fell at the same time. On the whole, it is possible that no insignificant amounts of rain may be lost in the course of the year, by manipulation at the time of measurement, if one has only a single instrument to deal with and cannot put another in its place. The junction of the collector with the receiver should be quite water-tight for certain very heavy falls (*wolkenbrüche*), in order that the water which falls, over and above the full of the collector, may at least remain in the gauge.

Of my two gauges exposed at the same time, that with a square aperture constantly gives greater quantities than that with a cylindrical aperture. The receiving area of the former is, however, only about quarter of that of the latter.

For the time of measurement I recommend noon, as it chances to come at the time of least fall, at least in summer, and has some relation to the divi-

* Jahresbericht der Kon. Sternwarte bei München. 1852.

sion of time, as the astronomers begin the new day at that hour. I should certainly prefer midnight if the hour were not too inconvenient.

12. I should not distinguish between the days of rain and snow, or if that be done, should always give the number of days on which a fall occurred, for then you have a safe check in the rain gauge, and because it is in many cases difficult to separate the days of snow from those of rain. If, however, the nature of the fall is given, in the way adopted at the Central Anstalt in Vienna, all requirements are met.

Separation of rain from snow, p. 40.

13. I consider the separation of the falls of hail from those of "*graupel*" still more difficult. "*Graupel*" consists of balls of snow, and, as such, is almost always found as the nucleus of the hailstone, which has a shell of ice. This latter may, however, be infinitely thin in many cases.

Separation of "graupel" from hail, p. 40.

We know also that hail falls principally in summer, "*graupel*" in the spring months. If in addition we remember that, in winter, snow-"*gries*" falls, the smallest form of "graupel," we can no longer hesitate to consider all these forms as simply different modifications of a definite form of precipitate.

14. I consider the enumeration of the thunderstorms, as such, as impracticable, and I succeeded in carrying my point on this subject at the Statistical Congress at Vienna in 1859. It would be nearly the same thing as trying to count the number of gusts of wind.

Thunderstorms, p. 41.

When does one storm end and another begin? On a day with constant rain it lightens and thunders at intervals for an hour together; how many storms are there? A storm in one place produces another elsewhere, and so forth; one of them is therefore near to, and the other at a distance from, the same station; should both be counted? Should all the storms be counted which occur on a hot summer's day, at a greater or less distance from the observer, and either simultaneously or successively in an isolated rain-cloud?

How different will be the result of counting at two stations not far asunder, which ought to agree with each other approximately in their numbers. What phenomenon is to be taken in all cases as the beginning or end of a thunderstorm?

On the contrary, how easy and definite is the counting of the days of storm. It is enough to have had one observation, of either lightning or thunder, to put down the day as a storm day. If more minuteness is required it would be better to count the hours of thunderstorm on each day, or even the electrical explosions themselves.

I think it just as difficult to separate the days of sheet lightning from those of thunderstorms; in the overwhelming majority of cases, though not invariably, sheet lightning is a thunderstorm at a distance.

15. For the measurement I recommend a vessel similar to the rain gauge but shallower, and with this difference, that the receiver is furnished with a cock, in order to measure the residual water in the measuring glass of the rain gauge. It should not be protected from sun and rain, but we should provide against the influence of insolation, in undue heating of the immediate vicinity, and should take care that the actual amount of rain, to be subtracted at the time of measurement, should fall into the vaporimeter. Schenzl at Ofen and Prettner at Klagenfurt use a vaporimeter of this kind.

Evaporation, p. 41.

16. The greatest trouble, in the estimation of the amount of sky covered, is given by the *cirri*, which generally cover a great part of the sky, and yet are so thin that, when the sun is behind them, only a slight diminution of insolation is produced, and very rarely is this quite intercepted.

Proportion of cloud, p. 41.

Inasmuch as practically they are only estimated according to their true extent in the latter case, at least when there are no other denser strata of clouds to be noticed, they should only be regarded at all whenever the last-named phenomena occurs, which is certainly very rarely.

There are similar difficulties with ground fogs, which are often so dense that, when the sky is otherwise clear, the sun and moon are obscured. The difficulty is still greater when this fog is in the act of dispersion and the blue sky shines through it. *Cirrus* and *stratus* should be therefore entirely disregarded in the estimation, but might be noticed, as observations in the journal, instead of the amount of sky covered.

It would certainly be best of all if photometric measurements enabled us to dispense with the estimation of the proportion of sky covered.

Proportion of cloud, &c., p. 41.

I should vote for the introduction of generally understood designations for the clouds, hydrometeors, and thunderstorms, if there were any prospect of their general adoption. If not, I recommend the designations employed at the Central Anstalt in Vienna, which were adopted at my suggestion.

Other extraordinary phenomena should be excluded from these rules of nomenclature, and in this respect freedom should be allowed to each observer.

Extra observations, p. 42.

17. I should wish to apply the last remark to this question also.

Uniform times of observations, p. 20.

18. I fear that the introduction of uniform periods of observation, if that means similar hours of observation, would meet with great difficulties, and would bring about the result of a diminution of the number of observers.

Inasmuch as the methods appear sufficient to reduce the observations, at least in the mean values, to similar times, it hardly appears necessary to aim at such uniformity.

The question is different if individual dates of observation are to be compared. In such cases similar terms of observation would be certainly very desirable. However, among a great number of stations, we can always find groups with similar hours.

Inspections and verifications, p. 23.

19. The verification of instruments and the inspection of stations should be considered as pertaining to the internal management of the respective central observatories.

Calculation of means, p. 25.

20. The mean values of the several meteorological elements should always be reduced to 24 hourly or true means. In the publications, however, the method must be explained in detail, in order to afford subsequent investigators the necessary opportunities for improvements, as far as the prospects of perfecting the methods of reduction will allow.

The monthly and yearly periods can hardly now be given up. It is another question, whether or not daily means are preferable to five-day means.

If no means are deduced for the separate seasons, the year should begin with January 1st, if the contrary be the case, with December 1st.

Calculation of normal values, p. 28.

21. All that has just been said is of even greater weight as regards the normal values, which should be calculated every five, or better every ten, years, or may be improved in certain respects.

Uniformity of publication, p. 29.

22. The publication of the meteorological observations of a limited number of stations in each country on a uniform plan, and within a comparatively short time, I think possible, but not desirable, apart from special objects.

Exchange of publications, p. 31.

23. The exchange of meteorological publications of different institutions and different countries will be conducted most simply and safely, if not always most cheaply, by the post.

Establishment of central offices, p. 31.

24. It is desirable that one, and even several, central stations should be established in each country for the management, collection, and publication of meteorological observations; but above all things care must be taken to have men fit to take charge of such establishments.

Weather telegraphy, p. 32.

25. A more extensive development and more settled organization for the exchange of weather telegrams seems undesirable, judging from the results already attained, at least in the countries at a distance from the sea.

Plans for Congress, p. 34.

26. The directors of all the central institutes should be requested to carry out the resolutions and the ideas of a Meteorological Congress.

V.

Dr. H. Hildebrandsson, Professor at Upsala.

Upsala, August 1872.

As I am unfortunately prevented from attending the Meteorological Congress at Leipzig, I cannot but comply with the wishes of the gentlemen who have issued the invitation, and therefore indicate briefly a few questions which I consider worth the attention of the International Congress.

Modern meteorology may be divided into four great branches. 1. The Meteorology of the Sea. 2. Climatology, including the theory of the instruments and the study of the regular Periodic Variations. 3. The Non-Periodic

Variations, or the general disturbances of the atmosphere, with their practical consequences; Storm Warnings and Weather Prophecies. 4. The non-instrumental observations, viz., those of phenological phenomena, of thunderstorms, of ice, of frosts, &c.

The first branch does not appear, from the invitation, to be brought into special notice. The brilliant results to which the uniform system introduced at the Brussels International Congress has led, appear to be a strong inducement to introduce a uniform system in other inquiries relating to meteorology.

On the other hand, most of the questions which have been proposed belong to the second branch, and the greater part of Dr. Buys Ballot's "Suggestions on a Uniform System of Meteorological Observations" treats of matters connected therewith. On this point I would only add one remark. It would doubtless be best if the same units of scale and the same instruments were used in all meteorological measurements; probably, however, such an arrangement would be fraught with great difficulties. Even if we might hope that national vanity, as a thing unworthy of men of science, would not come into consideration, yet among other matters it is a money question of no small importance to change all the instruments for others.

If, however, we suppose that such a uniformity in measures and instruments could be attained, an exact comparison of the observations of different countries would be very difficult, partly owing to the dissimilarity in the standard instruments, partly owing to the local conditions and situation of the stations. I would therefore propose the following proposition for the consideration of meteorologists:—

Would it not be of importance to nominate an international commission, with instructions to compare the normal instruments at the central establishments and the most important observatories in the various countries, and to determine definite rules for the reduction of the measures, scales, and nomenclature adopted in various countries?

The third branch of meteorology, or the study of the General Movements of the Atmosphere by means of simultaneous observations over as large a part of the earth as is possible, is without doubt that which presents the most important subjects for consideration to a congress of meteorologists. It is *only* by the combination of the efforts of all that the most important results can be attained.

Much has already been done in Paris. M. Marié-Davy has himself lately insisted on the necessity of further perfecting of the synoptical charts. In America synoptical charts are published three times a day, under the direction of Brigadier-General Myer. *If the same were done for Europe and the entire Russian empire, in Paris and St. Petersburg, a great step would have been made towards the solution of the most important theoretical and practical problems of our science, and Le Verrier's great plan would have been carried out.*

But the synoptical charts alone are certainly insufficient; they must be accompanied by observations of all the instruments. In this respect Question 22 of the invitation seems to us of the greatest importance. Buys Ballot has proved, by the admirable representations in his Jaarboek, that it is "useful and possible to publish the meteorological observations of a limited number of stations in each country in a uniform style and within a comparatively short time." *An enlargement of this admirable publication appears to us the easiest solution of this problem.*

For the study of storms, however, the records of self-registering instruments are of special importance. They afford us the means of recognizing the individual details of a disturbance of which the synoptic chart gives the general view. I have lately endeavoured to investigate this question in a small paper, "Etudes sur quelques Tempêtes," Gottenburg, 1872. The magnificent publications in the London Quarterly Weather Report are of prime importance in this respect, but unfortunately they only embrace British stations. If we had such graphical representations from the continent also, there is no doubt that we could study the changes in the atmosphere more thoroughly than by means of the synoptic charts alone. I ask, therefore, *Is it desirable and possible, in addition to the daily synoptic charts, to obtain also a graphical representation of the course of all the registering instruments at* 10 *or* 12 *stations in Europe daily, and in a short time after the observations?*

Let us now proceed to the fourth branch. The invitation has not included any question bearing on it. Such investigations are, however, carried out in many countries, and results of the greatest importance are being attained. These studies are carried on at a comparatively trivial cost, and they have the invaluable advantage of spreading in wider circles the interest for inquiries bearing on natural science. In many countries, especially in Austria and Belgium, phenological observations, of the highest scientific and practical value, have been instituted for a long period. In France and Norway the observations of thunderstorms have led to unexpected results. In Sweden also about 300 observers are engaged in recording the Ice conditions, Thunderstorms, and injurious Frosts. At our suggestion the Agricultural Association has named the observers in each province, and these are now in immediate relations with this observatory. The results of the Ice observations in the winter of 1870–71 are already published, and the Thunderstorm observations of the previous summer are now in process of discussion. How much greater and more comprehensive would the results be if such inquiries could be carried out in common over whole quarters of the globe. I venture, therefore, to propose the following questions to the Congress:

Is it of importance to organize in all civilised countries stations of the third order, without instruments?

If so, what observations should be made at these stations?

Is it desirable that different classes of these observations should be discussed and published at different central stations?

VI.

Capt. Hoffmeyer, Director of the Meteorological Institute, Copenhagen, to Director C. Bruhns, in Leipzig.

Copenhagen, August 9th, 1872.

I have to thank you for the kind invitation to the preliminary Meteorological Conference in Leipzig which I have received. I am unfortunately obliged to tell you that it will not be possible for me to leave Copenhagen.

The numerous arrangements which have to be made in connexion with the organization of the meteorological system in this country, and especially the testing of the different instruments which is now in progress, will not allow me to quit my post.

At the end of your invitation you have requested me to send in communications in writing, and so I allow myself to append notes on some of the questions proposed by you:—

Uniformity of scales, p. 11. Barometers, p. 13.

1. We use only millimetres and Celsius.

2. For stations of the second order we have arranged to employ Kapeller's station barometers, and aneroids are not used.

Exposure of thermometers, p. 14.

3. The thermometers are set up in a wooden louvred screen facing north.

Maximum and minimum thermometers, p. 17.

4. We hope to get maximum and minimum thermometers from Geissler, in Berlin.

Earth temperature, p. 19.

6. We intend to try experiments with Regnault's new air thermometer (silver capillary tubes).

Hygrometry, p. 34.

7. Although observations with wet and dry bulb thermometers are much influenced by local and other circumstances, we know of no better arrangements for stations of the second order.

Direction of wind, pp. 36, 37.

8. In my opinion very light winds had best be disregarded, because the observer is often led astray by the vane in such cases.

I do not think Lambert's formula satisfactory on the whole; at least the force must be taken into consideration therewith.

Scale of wind force, p. 38.

9. The scale 0–6 is the easiest for an observer to understand, but it must be understood that the force 6 virtually never occurs with us.

Wind velocity, p. 39.

10. We are thinking of introducing a simple apparatus for the measurement of wind force at stations of the second order, mainly consisting of a disc which

turns about a horizontal axis. By this means the indications of the several stations are at least intercomparable.

But what velocity will correspond to force 6 of the above scale?

11. An original, very simple, and durable rain gauge has been constructed here by Hr. Docent Fjord after a series of accurate experiments lasting many years. With its stand it costs only 9*s*. Rain gauges, p. 39.

What is, however, the best form of snow gauge?

15. At the instance of the sub-director of our institute, Hr. P. La Cour, rational experiments on evaporation are being carried on. At present I shall only allude to the importance of recording the temperature of the vaporimeter. Evaporation, p. 41.

16. Universally intelligible symbols appear to me very desirable. Proportion of cloud, p. 41.

17. The observation of the motion of cirrus clouds is certainly of extraordinary importance. Extra observations, p. 42.

20. The division of the year (Dec.–Dec.) into 12 periods of 30 or 31 days, and of these periods into five-day intervals, of which some contain six days, which Buys Ballot adopts in his Jaarboek, seems to me exact enough and suitable. Calculation of means, p. 25.

22. This is certainly of great importance, because it makes it possible to discuss recent meteorological occurrences, and also the general character of the weather in adjacent countries, and thereby increases the interest of the public, as well as adding zest to the labours of the scientific man. Uniformity of publications, p. 29.

The monthly observations of our stations will be published if possible on the 15th of the following month.

25. The weather telegrams will soon be as necessary for sailors and farmers as Exchange telegrams are now for commercial men. A better distribution of the stations, a firmer international organization, and above all things a more certain and rapid transmission of the despatches by wire, would be very desirable. Weather telegraphy, p. 32.

In an international organization the difference of local time must be considered (as is already the case in North America).

I add further:—

1. On our thermometers 95° is engraved instead of—5°, 90° instead of—10°, &c., in order that the observer may always read from below. Many errors are certainly avoided hereby.

2. Stations of the third order appear to me very necessary. The choice of stations of the second order is often determined by circumstances other than those which are purely meteorological (such as the proximity of a telegraph station, the existence of a skilled half-volunteer observer, &c.) Hereby it is not unusual to find the wind conditions somewhat disturbed, at times also the temperature and rainfall. Stations of the third order (without barometer or psychrometer) can be established much more easily at stations where the meteorological conditions are less local.

VII.

Engineer Lauterburg, formerly Chief of the Central Hydrometrical Bureau of Switzerland.

Berne, August 8th, 1872.

You have honoured the undersigned with a circular invitation to the European Meteorological Conference in Leipzig, and have thereby laid on him the obligation of returning you his sincere thanks for your kind attention.

In thanking you I must first of all express my great satisfaction that an attempt is at last to be made to carry out a uniform and systematic procedure in the organization of meteorological observations, although I myself have never *personally* engaged in meteorology, because I was only in the position of *turning to account* the results of meteorological observations in various branches of science and art, of theoretical and practical importance (such as drainage and hydraulic engineering).

As the undersigned could not hope to contribute material direct assistance to the Conference by his attendance at it, it will afford him the greater pleasure to be able to assist it indirectly to the extent of his power, inasmuch as he will urge on the Government all the suggestions which may be addressed by the Conference to his countrymen, in his capacity of member of the Swiss Naturforschende Gesellschaft, at its meeting at Freiburg, August 19–21, as well as in virtue of his former presidency of the Central Hydrometrical Bureau. Although in consequence of a great increase of work he has had to hand over to the Swiss Inspector of Works, with a large collection of charts and views, the hydrometrical functions which he had called into existence and organized in Switzerland seven years ago, his influence with the Federal Officials in regard of this branch of investigation, immediately dependent on meteorology, is by no means at an end.

In order to give the Conference in a few words some idea of the mode of application of one of the chief results of meteorological observations, viz., the rainfall, I enclose a copy of a River formula ("Stromverbal") for one of the small rapid mountain streams. The data which have been taken from the meteorological observations refer to articles 7, 8, and 9.

In the same way the undersigned has calculated the mean minimum and maximum volumes of all the Swiss rivers of the first, second, and third rank, and published them in comparison with the immediate results of current measurement in a small pamphlet sent herewith. The method in which the main factors of the rainfall have been deduced in it shall, if possible, be explained in a subsequent pamphlet.

VIII.

Dr. H. Mohn, Director of the Royal Meteorological Institute of Norway at Christiania.

Bergen, August 1st, 1872.

When I received the circular with the invitation to the Meteorological Conference of this year in Leipzig, I had already arranged my tour of inspection of the meteorological stations for the year, and I could not think of going to Leipzig, however much I should have wished to do so.

Now that I am on my tour I have not leisure enough to devote such a thorough consideration to the Meteorological Conference as I should otherwise have given to it. However to show my interest for this question, which is of such universal importance, I allow myself to express in a few words my views on several of the questions proposed in the circular.

Uniformity of scales, p. 11.

1. Metrical scale and Celsius degrees everywhere; no reduction.

Barometers, p. 13.

2. Aneroid barometers are *not* to be used for fixed stations.

Exposure of thermometers, p. 14.

3. Something like that introduced in Scotland. In this country the thermometers at the telegraph stations must be set up outside the window of the office, else the telegraph staff could take no observations.

Hygrometry, p. 34.

7. For the observer the psychrometer is the most convenient. In severe cold the hair hygrometer must be used with it. The convenience of the observers is an important point to be considered, in order to obtain trustworthy observations. We could obtain no results with complicated hygrometers. The psychrometer is certainly untrustworthy in cold (below -20°C) and in strong winds. We shall obtain from Russia information as to the value of the hair hygrometer in such cases.

Direction of wind, pp. 36, 37.

8. By the English initials, no numbers. Lambert's formula should not be used. The force 0 should not be included.

Scale of wind force, p. 38.

9. Numbers 0–6, the half of the Beaufort scale. A small anemometer, something like a plate moving round a horizontal axis, would render good service for measuring the force, and ensuring uniformity in measurement.

Velocity of wind, p. 39.

10. Good in open localities. Kilometres per hour.

Rain gauges, p. 39.

11. Not heavy, for the observers will not bring heavy masses of snow into the houses. It would be best to make the measurement after each fall, and in the mornings. At our Arctic stations the measurement of snow is often quite

impossible and illusory. The snow flakes do not fall into the rain gauge, or else the snow which has already fallen is drifted by the wind, and mixed with that which is falling subsequently.

12. I have separated them. Separation of rain from snow, p. 40.

14. I have tried both methods. Perhaps it is enough to count the days. Sheet lightning separately. Thunderstorms, p. 41.

15. No experience. Evaporation, p. 41.

16. Blue sky = 0, overcast = 10. Symbols would be very convenient. Proportion of cloud, p. 41.

17. The so-called atmospheric electricity belongs as yet to Physics. Extra observations, p. 42.

18. The time of observation must be arranged according to the convenience of the observers. Uniform times of observation, p. 20.

19. With difficulty. Inspections and verifications, p. 23.

20, 21. The length of the intervals depends on the working power of the institute charged with the reduction. Monthly means should be set in the first place as regards importance, and should always be given. The method of calculation depends on so many circumstances that it can hardly be discussed unless verbally. Calculation of means, p. 25.

22. According to my experience, yes. Uniformity of publication, p. 29.

23. Printed observations which are not collected by telegraph, by book post every month. Exchange of publication, p. 31.

24. Yes. They exist in almost all countries of Europe. Establishment of central offices, p. 31.

25. Yes. Especially for storm warnings. Weather telegraphy, p. 32.

I shall be extremely interested, on my return to Christiania, which will be about September, to learn the conclusions at which the Meteorological Conference at Leipzig has arrived, and I venture to tender to the Conference my heartiest wishes for the completion of its arduous tasks, and to present my best compliments to my honoured colleagues who attend it.

IX.

Letter from Sanitäts-rath Dr. A. Mühry at Gottingen.

July 1872.

I recommend to the Meteorological Conference five meteorological questions which seem to merit general consideration.

1. The *general storms of Europe*, which usually occur five or six times a year, especially in winter, and travel from the westward, *i.e.* in the Antipolar Current, seem, in respect of their character, extent, and direction, to be only capable of becoming generally known by means of systematic observations carried out on a uniform system for at least some years.

2. The *temperature of the soil* has been sufficiently ascertained only in the middle latitudes; but in order to establish the theory, comparable observations are wanted in the Torrid and the Frigid Zones. A few years' observations, conducted on a uniform system, are required.

3. The *temperature of lakes* in depth is scarcely known, except in the neighbourhood of the Alps, and here too, hardly in winter; besides, it has been determined in a few of the Arctic lakes.

It would be desirable, for the theory, to determine the temperature, in depth, of a lake near the Equator, where it probably would hardly differ from the surface temperature.

Moreover there is a want of investigation of the temperature of salt lakes, with at least 3·3 per cent. of saline contents, in order to be able to decide by experiment if their behaviour is the same as that of fresh-water lakes, and to make use of the results for the theory of the conditions of temperature of the ocean.

4. The modern, if we may so term it, *the intuitive meteorology*, must wish, that in the arrangement of the reports of the observations which have been made, care were taken by means of combination (comparison) of the separate

elements to facilitate the attainment of a general comprehensive view of the meteorological conditions as a whole, so as to be able over the whole extent of Europe to recognize and trace out the simultaneous positions of the Trade Winds, and the changes which they undergo.

5. The choice of localities on the earth's surface suited for the *astronomers* to observe the *transit of Venus* in 1874 would be worth consideration from a meteorological point of view, in order to ensure the result, by avoiding a climatic cloud covering.

X.

Letter from M. Cecilio Pujazon, Director of the Marine Observatory at San Fernando, to Director Bruhns.

San Fernando, August 7th, 1872.

Contrary to my wishes, it is impossible for me to attend the Meteorological Conference in Leipzig, but I hope to be able to travel to Vienna next year, and I shall be very glad to take part in the Congress there.

Meanwhile I am ready to adopt all the conclusions of the Leipzig Conference, inasmuch as it is only by unanimity and uniformity in all our investigations that any progress can be made in meteorology.

XI.

Letter from Prof. Ragona, Director of the Royal Observatory at Modena, to Director Jelinek.

Modena, October 7th, 1872.*

At the meeting of August 14th I greeted the first International Meteorological Conference, and recommended to the Congress two points for consideration for the advancement of the science.

The first bears on the influence which the Congress should exert on such European powers as have not yet established a central physical observatory or a meteorological institute, in order that the deficiency which exists might be supplied according to a plan suited to the conditions of the locality, and which should be proposed by a commission appointed for the purpose.

The second matter concerns the publication of a compendious Handbook and Text book of Meteorology, a real encyclopædia of meteorological science.

As this work should represent the latest position of meteorology, down to the most special details, on the basis of the discussion and calculation of the most important observations carried on over the whole surface of the globe, so that it should serve as a point of departure for future inquiries, and as a scientific basis for future labours, it is self-evident that a treatise so copious would exceed the powers of a single author. The duty of sketching out the general plan and the division of the work should be entrusted to a special commission, and then the most eminent meteorologists of the old and new world should be invited to take part in the editing of the respective subdivisions.

Barometers, p. 13.

2. With reference to aneroid barometers, my opinion, based on a long series of observations, the results of which I published in Secchi's "Bullettino Meteorologico," is to the effect that this instrument can only replace the mercurial barometer, and serve for the determination of barometrical maxima and minima, when it remains constantly at the same observatory, and when the corrections depending on the temperature and the condition of the aneroid

* Professor Ragona, who took part in the preliminary conference at Leipzig, has by letter expressed his wish that his remarks should be expressed at somewhat greater length, and moreover he has treated some of the questions of the programme more fully. It did not appear possible to introduce his letter into the report, without making an exception to the uniform treatment to which the views of the respective speakers were necessarily subjected, and so the wishes of Professor Ragona are met by introducing his letter in this place.

have been accurately determined. The corrections of the aneroid are liable to change if it be moved from place to place, so that at each new locality a fresh accurate comparison and determination is required; so that in its present form the aneroid cannot be recommended as a substitute for the mercurial barometer not even for meteorological stations of the second order.

3. In respect of the exposure of thermometers, I draw a distinction between that in an open place exposed to the sun (as in a garden or on an open terrace), and the exposure usual in Italy in a sufficiently large window of a high building and facing North. In the latter case I recommend the system which I employ, the description of which is already published, and which has proved satisfactory in practice. Exposure of thermometers, p. 14.

In order to shelter the thermometers from the influence of the sun's rays, of the rain, and radiant heat, &c., I recommend that only such means as are absolutely necessary should be employed, and I object to all other arrangements, such as double or triple louvres and roofs. I must also state that I object to all arrangements which alter the thermometrical condition of the air, such as the employment of artificial currents of air, produced by aspiration or pressure. The temperatures obtained by all such means are artificial, and may differ materially from the real temperature of the air.

I am convinced of the importance of some movement in the air near the thermometer, in order to remove the film of air which adheres to the instrument, but I prefer, to the rotation of the thermometer, a slight movement of the air, which is produced at the Italian stations by a fan.

4. I consider as the main condition for trustworthy results from the maximum and minimum thermometers that the instruments in question should be placed as near as possible to the thermometer intended for the observation of the temperature of the air at every observation. Maximum and minimum thermometers, p. 17.

As the temperature of the air from time to time, as well as the maximum and minimum, are only expressions of the changes which the same phenomenon undergoes in a definite period, it is easily seen that, if these changes are observed on instruments situated in different strata of the atmosphere, such observations will not admit of a strict comparison, as has already been shown, by direct experiment, both by Lamont and myself.

From this point of view, the old maximum and minimum thermometer of Bellani might be recommended, because that in order to determine the air temperature by it you read, on the scales, the level of the liquid columns instead of the position of the indices. It has, however, gone out of use because it does not bear carriage well, and easily gets out of order in other ways. I am, however, of opinion that science would gain if Bellani's instrument could be improved, and substituted for the dry thermometer in the psychrometer (as is done by the Tecnomasio Italiano).

In this case you could obtain from the same instrument, of which the thermometer bulbs are very close together, and are therefore situated in the same stratum of the atmosphere, the data for air temperature, vapour tension, humidity, and the extremes of temperature.

5. For the determination of the temperature of insolation I recommend an instrument proposed by me in the year 1862, at the Polytechnic Association in Turin. It consists mainly of a heliostat with clock work, which carries a maximum thermometer of Negretti and Zambra's constantly at right angles to the incident rays of the sun from sunrise to sunset. Such an instrument must of course be set up in a locality which is perfectly open and exposed to the sun's rays at all times of the day. Radiation, p. 19.

7. For the determination of the moisture of the air I most warmly recommend the psychrometer constructed by the Tecnomasio Italiano in Milan, and I would point attention to the observations of Belli and Cantoni, which show that false results are obtained, if psychrometers of other constructions are employed, especially thermometers with spherical bulbs, covered with linen, and moistened by a thread constantly dipping in water. Hygrometry, p. 34.

With reference to the hair hygrometer, I can prove by the experiments instituted by me and published in Secchi's "Bullettino Meteorologico," that this instrument gives quite as good results as the best psychrometer, if its corrections be first accurately determined, by comparison with the psychrometer.

Moreover the hair hygrometer may be arranged as a self-registering instrument by employing a bundle of 30 or 40 hairs.

It is a great advantage to enclose the hygrometer in a screen of the finest wire gauze.

Wind, pp. 36 37, 38, 39.

8, 9, 10. Lambert's formula in its original form is useful even for observatories of the first order, as it leads to the recognition of special principles, the investigation of which bring instructive and useful results to light.

At all events it would be very desirable that cheap anemometers should be made, which should give the wind in kilometres per hour, could be easily fitted, and should meet the requirements of a meteorological station of the second order.

The present use of the wind scale, as we find it at most of the smaller observatories, is as untrustworthy as the determination of temperature by means of thermometers exposed at different places.

Each observer describes the wind which appears to him strongest, by the highest figure of the scale; but the intensity of this wind is very variable in different localities. The strongest wind felt in a town in central Europe is not the strongest wind which occurs in an island in the West Indies. If *e.g.* a scale of 8 parts (0–7) be made the basis, it may easily happen that the wind described as 7 may mean for one observer a velocity of 60, for another one of 80 kilometres.

Rain gauges, p. 39.

11. In the Royal Observatory at Modena the measurement of the rainfall, &c., is made at midnight in order to ensure uniformity with the civil reckoning.

In cases of heavy falls the collecting vessel is frequently emptied, but only the sum of the amounts registered is entered in the register.

In addition, the beginning and end of the rain, as well as its intensity, are registered by the pluviometer, which records by electricity.

Evaporation, p. 41.

13. In respect of evaporation I can refer to numerous experiments which I have made on the subject. I recommend for adoption my method of observation, which consists of two vaporimeters of equal areas erected at different heights, of which one is in the open air, the other is at the north window of the observatory (the *finestra Meteorologica* of the Italians). The observation of the lower vaporimeter is always possible, while that of the upper one may often be vitiated by wind, rain, and other agencies. On suitable days comparative observations are instituted between the two instruments, which serve for the determination of the factor for reducing the lower instrument to the upper one.

I can recommend for use the vaporimeter made at the Tecnomasio Italiano in Milan, and, wherever a greater outlay is not an objection, the self-registering vaporimeter invented by myself.

Extra observations, p. 42.

17. The observation of atmospheric electricity seems very desirable, and it would be well to introduce it at a greater number of stations.

I recommend, as an observing instrument, Palmieri's Bifilar Electrometer, with the improvements introduced by me.

However it is to be wished that the self-registering electrometer may soon be so far improved as to be capable of more general adoption.

Uniform times of observation, p. 20.

18. It is very difficult, even in smaller systems of observation, to ensure uniformity in the hours of observation.

It will therefore be necessary to disregard this uniformity altogether, and rather to aim at obtaining the uniformity by reductions, which may be easily obtained from the normal values known for the separate stations. Hence follows the necessity of the establishment of a central observatory in each country, the duty of which should be to determine the normal values for the separate stations by differences which should be based on observations of such stations as have a long-continued and accurate series.

Calculation of means, p. 23.

20, 21. I am of opinion that the meteorological year should begin with December, and be divided into four periods of three months each, inasmuch as it does not seem right to begin a series with a maximum or a minimum, and, by the mode of division proposed, the maximum and minimum as well as the mean temperature will fall in the middle of each period.

The means may be calculated for decades, months, seasons, and years, but yet I recommend especially the calculation of five-day means, whereby the system introduced by Dove should be used for all stations and without deviation.

On the other hand, the five-day means which begin with each month, such as are given by some observatories, should be rejected, as there are always irregularities, with the last of these means, in regard of the number of days, as it may consist of three, four, five, or six days. Such means have no value, and cannot be compared with Dove's five-day means.

22–24. With reference to the question as to the establishment of meteorological central observatories in every country, and the publication of meteorological observations, which should be carried out as rapidly and completely as possible, I refer to my remarks at the commencement, and I consider the utility of such institutions so evident that it makes all discussion of the subject superfluous. Organizations, pp. 22, 31.

XII.

Professor R. Rubenson to the Meteorological Conference at Leipzig.

Upsala, August 1872.

The writer of these lines, being prevented from taking part in the Meteorological Conference at Leipzig on the 14–16 August this year, and having a sincere conviction of the importance of all measures, the aim of which is to bring about a greater degree of uniformity in the meteorological labours of different countries, and a more unanimous agreement on the principal questions of the science than has been the case hitherto; takes the liberty of responding to the request made to meteorologists to communicate, if only in writing, their ideas on the easiest and surest means of attaining the end desired.

1. Whoever follows attentively the ever-increasing literature of meteorology cannot fail to perceive that works bearing on the meteorology of limited districts are in reality those which pretend to the greatest accuracy, while works of a more general grasp become less sure in proportion as the district to which they refer becomes more extended. Up to a certain point it is in the nature of things that this should be the case; greater exactitude ought evidently to be attainable in the comparison between neighbouring localities, which necessarily present less differences in a climatological point of view, than when the distribution of a meteorological element is studied over a larger area. But while admitting that it should be so to a certain extent, it by no means follows that the result of an investigation on a more extended scale cannot be made relatively quite as certain as that of a limited region. In other words, the uncertainty of the quantitative data ought, even in this case, to lie within predetermined limits, depending on the nature of the problems which the general investigations in question are called upon to solve. Now, if we examine the results arrived at in the investigations of whole portions of the globe, and *á fortiori* of the entire northern hemisphere, and if we inquire, for instance, with regard to the temperature, the height of the barometer, the wind and rainfall, what degree of accuracy is obtained by the charts constructed for all these phenomena, the reply will be, unfortunately, that they can only be considered as the first sketches of the system of climatological cartography at which meteorology is aiming. If we pass to other meteorological elements, such as humidity, evaporation, amount of cloud, &c., there is no chart existing which embraces an extensive area. These circumstances should not be attributed to the disparity of attention paid to the observations in different countries, for they seem to me rather to depend much more on the difficulty of grouping materials in all respects suitable for investigations of such large dimensions, and on the uncertainty which is still inherent in every combination of observations made in different countries; but I think they are chiefly caused by the large gaps which still exist in the principal systems of observation, in consequence of which the course of the curves over vast regions, both of land and sea, has become

sufficiently problematical. If *savans* like Dove and Buchan have not shrunk from the unheard—of labour of representing cartographically the monthly temperature, pressure, and wind direction for the entire surface of the globe, if they have well-earned claims on the gratitude of meteorologists, it is nevertheless impossible to deny that owing to the above-mentioned difficulties, their charts cannot give that complete knowledge of the climate of the entire earth which is absolutely necessary for the solution of all the fundamental questions of the science.

If we study the question of the diurnal variations of the meteorological elements, a question which has occupied meteorologists at least from the beginning of this century, we shall find that, in so far as certain localities are concerned, the laws of these variations are pretty well known, thanks to the arduous labours of many meteorologists. But as soon as we wish to find the difference between these variations at different latitudes, their dependence on the continental or maritime situation of the locality, their modifications, with height above sea, &c., our knowledge is limited to the most general qualitative differences; it is all but impossible to give the numerical relations with sufficient exactness to be generally adopted. We then find again here the same great gaps which must be filled up before general laws for the phenomena in question can be established. Our ignorance, respecting the extent to which several of the diurnal variations are applicable, has often led to a proceeding which is generally acknowledged to be incorrect, but which, it seems to me, is much more frequent in this branch of meteorology than in others, namely, that of deducing from the observations made at a single point the meteorological conditions of other, very distant, places. The urgency for the most searching comparisons between the variations at numerous localities, is moreover proved by the fact that diametrically opposite opinions as to the causes of the greater part of these variations, have been enunciated by the most eminent meteorologists of our age.

If we pass to the branch of meteorology which, in recent years, has constituted the principal object of meteorologists, viz., that which treats of the "Non-Periodical Variations," as they were formerly called, or the "General Movements of the Atmosphere," as they are now termed, it appears to me that the difficulties again begin to be felt, as soon as it is a question of grouping together the observations of different countries. In order not to confound the two objects towards which all arrangements tend in this branch of the science, viz., the theoretical and the practical, I shall examine each separately, commencing with the latter.

We may assume it as an axiom that if, at intervals of sufficiently short duration, accurate information as to the state of the atmosphere over a vast area is communicated to a certain place at its centre, this intelligence may usefully serve for determining the modifications which the meteorological conditions will undergo at this same place, or in its vicinity, during that day or the next; and similarly, if the telegraphic arrangements allow of the communication of telegrams on the state of the atmosphere between the central station and the other points of the country, the latter may derive great practical advantage, always provided that the telegrams received at the central station are submitted to a serious discussion, and the telegrams issued by it are modified according to the situation and climatological conditions of the localities to which they are sent. Although all the obstacles connected with the certainty of telegraphic transmission have not been hitherto completely overcome, although more frequent telegrams are probably necessary, and as yet some uncertainty exists as to the best means of utilizing the reports dispatched from the foreign and home stations to the central office, we may, however, I venture to think, admit with sufficient certainty that in every country, which has a central institute of this kind, the difficulties referred to will soon be removed, as they may be attributed almost exclusively to the novelty of the system. The fact that this work may be executed with very great precision is proved, among others, by the remarkable synoptic charts published three times daily by the American establishment under the direction of General Myer. On the other hand, it appears to me infinitely more difficult to attain the requisite certainty as soon as the telegrams of different countries are combined. The most comprehensive work of this kind is that which is carried on daily at the Paris

Observatory, and which is undoubtedly based upon the grandest idea. But I cannot refrain from expressing the opinion that the practical object in view appears to me to require more accurate data, speedier telegraphic communication, with fewer interruptions, and more numerous stations in several parts of the area covered by the system of observation, &c.; in fine, that the Bulletin of the Observatory cannot yet be considered to have attained the complete perfection of which it is assuredly capable. The Observatory of Paris, which doubtless endeavours by every possible means to smooth away the difficulties caused by the above-mentioned disadvantages, itself acknowledges the want of improved and more complete charts, as it is easy for errors to creep into the original charts. These improved charts have, without doubt, great importance for the pursuit of theoretical researches, but they can in nowise dispel the inconveniences which may arise in the civil life of the different countries, from the fact that the establishment, which has undertaken to furnish each day a general view of the state of the atmosphere over the entire surface of Europe, has not at its command sufficient and perfectly accurate materials. When we know the pains which the Observatory of Paris has taken in the organization and maintenance of this international work, we are led, it seems to me, to the very natural conclusion that no central meteorological station, of any country whatever, can, when left to itself and its own resources, execute in an entirely satisfactory manner a work which requires the co-operation of every country of Europe.

If we now approach the domain of scientific investigation properly so called, and if we examine the resources at the disposal of the meteorologist who wishes to undertake a work on the "General Movements of the Atmosphere," it is incontestable that they have wonderfully increased during recent years, by the quantity of journals which have begun to be published in all countries and observatories, whilst the facility for obtaining them in time for use has also increased in a high degree by the rapidity with which they appear and are distributed.

The discussion of facts relative to meteorology over a large area, the whole of Europe, has met with the greatest assistance in the continuous abstracts on the meteorological conditions of Europe, published by M. Buys Ballot in his "Jaarboek." The collection of the materials required for researches of this kind is no longer accompanied by great difficulties, even if the discussion refers to a period so recent that the observations of all countries are not yet published, for even in this case it is easy to procure the necessary observations, thanks to the readiness with which they are communicated by the directors of central institutions and observatories of the different countries. Nevertheless we find that great difficulties stand in the way of the employment of observations gathered from all parts. Whichever method is used for rendering the observations intercomparable, either their reduction to sea level, or the calculation of deviations from normal values, there is always uncertainty, in the first case, about the height above sea of numerous stations, and, in the second, as to the real magnitude of the normal values. The latter are, as we know, much better determined for certain points than for others, but are entirely wanting for whole countries. Moreover, if we only use a part of the stations of a certain country, in an investigation embracing a large part of Europe, how can we be sure that we have selected those where local influences do not play too great a part. This circumstance has remarkable weight in countries where the stations are far removed from each other. Then follows the question of knowing whether the instruments of one country have been compared with those of other countries or not. It is easy enough for each individual country to effect this comparison, and no doubt it has been done wherever there is a central institution. But the comparison of instruments of different countries, which is requisite for scientific work of general grasp, has hitherto been entirely left to private meteorologists who are interested in works of this kind, and therefore must be considered as very far from having been done in a sufficiently general manner. Notwithstanding the great and real merits of M. Buys Ballot's work, a work which evinces the most strenuous efforts to render the observations of different countries intercomparable, I cannot help thinking that it would become of much greater importance if the stations included in it were most carefully chosen by the meteorologists of the

countries where they are situated, if their height above sea and their normal values were rigorously determined, if the instruments employed were compared with each other in as direct a manner as possible, if the wind observations were considerably increased, if only such values were entered as were perfectly warranted by the central institutions of the different countries, and lastly, if the network of observation were extended to Asia. If these conditions were fulfilled, the work in question, without any essential alteration of its plan, would certainly be almost exclusively used in every investigation of the general questions relating to the modifications to which the state of the atmosphere in Europe is daily subject.

I think I may conclude, from the preceding remarks, that the fact that the general results have not yet reached the necessary degree of accuracy, and that the number of comparative works of greater generality has hitherto, relatively speaking, been very limited, is mainly attributable to the difficulty of combining with certainty the observations of different countries, as well as to the want of suitably equipped stations over several regions of great extent. In reflecting on the best means of overcoming these difficultics, I am the more convinced that, except in some quite special cases, it cannot be done by the efforts of isolated meteorologists; but that which can only be effected partially, and in an incomplete manner, by individual *savans*, ought to be realized *by the united efforts of all meteorologists*.

The diametrically opposite theories, at present obtaining in several branches of meteorological science, and which may be attributed, at least in part, to the uncertainty still existing in our knowledge of meteorological phenomena, can no longer hold, it seems to me, if we can do away with the difficulties to which I have cursorily referred above. The best proof of the correctness of a meteorological theory is certainly its agreement with observations taken over the entire globe. It is in setting out from this point of view that I attach very special importance to the liberation of general phenomena from their present uncertainty. But as this unfortunate state of things can only be removed, it seems to me, by the united efforts of a great number of *savans*, I venture to call the attention of the Conference to this important circumstance, so that it may become an object of consideration at the Congress of Vienna next year.

Accordingly I take the liberty of proposing the following four questions for discussion at the Vienna Congress:—

1°. What measures should be taken for obtaining, in the shortest possible time, general charts of meteorological elements for which none exist at present, and for improving and completing those already constructed?

2°. How can our imperfect acquaintance respecting the diurnal variation of the meteorological elements at different latitudes, heights above, and distances from, the sea, be remedied in the shortest possible space of time?

3°. How can international meteorological telegraphy be rendered perfectly certain and serviceable in all the countries of Europe?

4°. Is it not desirable that M. Buys Ballot's excellent tables on the state of the atmosphere over Europe should be completed and extended to such a degree as to allow of their being used as a basis for all investigations of the "Non-Periodical Variations" over the whole of Europe, by means of special preparatory works of the meteorologists of other countries?

XIII.

Mr. G. J. Symons' Notes on some of the questions suggested in the invitation.

(The numbers refer to the questions.)

London, July 27, 1872.

(1.) I do not think identity of scale necessary, but I think that improved and extended conversion tables are so. I do not know of any better than Guyot's, and for the conversion of rainfall measurements they might be rendered more convenient than they are at present. Uniformity of scales, p. 11.

(2.) The form known as "Kew pattern;" I have a very high opinion of aneroid barometers, but only when they can be frequently compared with a mercurial barometer. Barometers, p. 13.

(3.) Under the partial direction and at the expense of the Royal Society of London I have for several years been at work upon this subject, and the discussion of the whole of the observations (about 120,000 readings) is nearly completed, and will, I hope, be printed in a few months. I annex (Appendix I.*) description of the experiments, and have only to add that should it appear desirable to try any other plans I would use my best efforts to see them carried out. Exposure of thermometers, p. 14.

(5.) I think that small-bulb mercurial thermometers with the bulb and one inch of the tube coated with dull black, the whole enclosed in a glass jacket thoroughly exhausted of air, and then mounted on a post so that the bulbs are freely exposed to the sun, and 4 feet above the ground, are the most strictly comparable instruments with which I am acquainted. Radiation, p. 19.

(7.) I do not consider the dry and wet bulb perfect, but preferable to Saussure's hygrometer. Hygrometry, p. 34.

(11.) I annex (Appendix II.) copy of rules generally adopted by British observers of rainfall. I think that the funnels should be circular and have strong rims; square funnels, and those not made strongly are more liable to distortion and to warp with the sun's heat With respect to the size of the orifice, very elaborate experiments have been made under my direction, and the result is to show that the variation from this cause is not more than one or two per cent. In actual practice I recommend gauges, whose orifice is 5 inches in diameter, for ordinary stations where observations are made daily, and 8-inch gauges for stations upon mountains and localities only visited monthly. We find in this country that 9 a.m. is by far the most usual and convenient (and therefore punctually adhered to) time of observation. Rain gauges, p. 39.

(12.) Yes, I think it is desirable. Separation of rain from snow, p. 40.

(13.) I think not. Of "Graupel" from hail, p. 40.

(14.) I should reckon the number of "days of storm," but heat-lightning being really a distant storm would hardly seem entitled to be included. Thunderstorms, p. 41.

(15.) Very elaborate and costly experiments on this subject are in progress at the expense of the Royal Society of London, under the superintendence of Mr. Rogers Field and myself; some information on the subject is given in Appendix III. Evaporation, p. 41.

(16.) By all means introduce such symbols if possible. Proportion of cloud, p. 41.

(20.) I think on the whole the 1st of January is to be preferred. Calculation of means, p. 25.

I venture to suggest that an international exhibition of meteorological instruments would be a useful feature of the Congress of 1873, but it must be far more complete than at any of the previous exhibitions if it is to be of service to meteorology.

62, Camden Square, London.

* Mr. Symons has forwarded with his letter two pamphlets (Appendix I. and III.), of which the first contains the various forms of thermometer-stands, the second those of vaporimeters, and also a sheet (Appendix II.) with rules for the observers in respect of the measurement of rain. These Appendices contain numerous woodcuts, which have already been printed elsewhere, (in the "Monthly Meteorological Magazine," and the "Reports of the British Association,") and cannot be reprinted here.

XIV.

Prof. Dr. R. Wolf, Director of the Observatory in Zürich, to Director Beuhns.

Zürich, August 1st, 1872.

In addition to other reasons, the determination of longitude between Gäbris, Zürich, and Pfändler, which is at present in progress, prevents my going to Leipzig to take part in the Meteorological Conference. Accordingly I allow myself to hand in my vote by this letter, which I request you to communicate in my name. In it I shall hold to the numbers of the programme and remark :—

Uniformity of scales, p. 11.

1. As units of measurement, millimetres and Centigrade degrees should be unconditionally maintained, and especially Reaumur and Fahrenheit degrees should be set aside wherever it is possible.

Hygrometry, p. 34.

7. The psychrometer always gives wrong results when frost and thaw alternate quickly, but not so the hair hygrometer. It is therefore requisite, at least at central stations, to let each instrument check the other.

Direction of wind, pp. 36, 37.

8. Lambert's formula is valueless.

Uniform times of observation, p. 20.

18. It is scarcely possible to introduce uniform periods of observation. If, *e.g.*, in Switzerland, our hours 7, 1, 9, were to be changed for 6, 2, 10, probably more than half of our observers, and at all events the majority of our mountain stations, would drop off.

Calculation of means, p. 25.

20. The meteorological year certainly begins more suitably with December than with January; I could only give my vote for January (*sic*).

Uniformity of publications, p. 29.

22. The style of publication must be left to the individual countries, as it depends on the special circumstances of the countries. *At most* the Meteorological Conference can name a minimum which is desirable, *e.g.*, wish, that not only means, but also at least two of the actual daily observations should be published for a number of stations in each country.

Plans for Congress, p. 34.

26. However desirable it may be that meteorologists should come to a mutual understanding as to what is *wished*, an object which, in my opinion, will be better attained by written than by verbal communications, they must guard against letting themselves be *ordered*. In order to carry out the views, *example* and *precept* are the best and the only practicable means.

APPENDIX B.

Proceedings of the Meteorological Section of the French Association for the Advancement of Science, held at Bordeaux, September 1872.

The *French Association for the Advancement of Science* held its first meeting at Bordeaux from Thursday, 5th September, to the following Thursday. It inaugurated the vast building erected for the Philomathical Society for the Instruction of Adults.

The hospitality of the town was worthy of this intelligent city, and a great number of the inhabitants took an active part in the labours of the association.

The Association, besides the general sittings, divided itself into four groups comprising 15 Sections. We have only to notice here the work of the Section of Meteorology and Terrestrial Physics. The programme had been previously drawn out, in conformity with the series of questions put by MM. Bruhns, Wild, and Jelinek, in the invitation to the Meteorological Conference at Leipzig. Other questions of special interest to France were added to them. We shall resume, question by question, the results of the deliberations, while

specially thanking MM. Linder, Raulin, and Belime for the devoted assistance they have rendered in this examination.*

1. Uniformity of measures is highly desirable; but even supposing it accepted in principle by every country, its complete realization would require a considerable length of time. It appears, therefore, immediately necessary to take steps for reducing the different measures to the same standard, and to make this reduction in the publications of an international character. Uniformity of scales, p. 11.

It is not equally necessary to introduce currently into the general science all the observations of any one country, which are issued in the local publications. The science must first rest upon a limited number of well-chosen and suitably equipped stations. It is of consequence that the observations made at these stations should be placed at the disposal of all meteorologists without delay, and in such a state of preparation as will render their use easy. The details of the science for some time yet may be left in each country to the exclusive care of its own meteorologists. During this time the incontestable advantages to be derived from the complete uniformity of measures will be more and more apparent, and the transformation will be effected without too much effort.

2. At all land stations it is desirable that none but mercurial barometers should be made use of; but Fortin's barometer may be simplified for stations of the second order, so as to reduce its price. There are barometers with a moveable cistern, graduated on glass, at the price of 35 to 40 francs. There are barometers requiring only one reading, graduated on glass or copper. M. Baudin has announced one, said to be very accurate and at an equally moderate price, but we have not yet received it from him. Our type barometer is still that of Fortin; not that its principle is the only one which may be accepted, and free scope must be allowed to the efforts of the makers. But it appears essential to us that each country should have at least one central establishment provided with a normal barometer which has been rigorously compared with those of other similar establishments, and the central observatory of each district should undertake the examination and verification of the mercurial barometers made in, or for, that district. Barometers, p. 13.

The section of meteorology expresses the wish that in France the Inspector General of Meteorological Stations will verify on the spot all mercurial barometers at the French stations and make their corrections known.

As to metallic barometers, they present serious defects, their zero is displaced in time and often in an accidental manner, and furthermore their temperature correction is uncertain. They should only be used therefore as weather glasses. From this point of view their use cannot be too much recommended or spread.

3. This is one of the most delicate questions in meteorology. The exposure of thermometers varies very much from one station to another, and each exposure gives different results. Let us add, however, that the differences affect the extreme temperatures much more than the means. Exposure of thermometers, p. 14.

In France the thermometers are generally sheltered by a double roof, sloping towards the south, and about a square metre in size. They are removed from buildings, and the ground, from which they are about two metres distant, is covered with grass. Abroad, they are often enclosed in screens made of venetian or louvre work. Then the radiation of the screen itself takes the place of that of the sky or ground.

In the present state of things it seems to us difficult to require the meteorologists of different countries to adopt immediately a uniform plan with regard to thermometers. We must first illustrate the advantages and disadvantages of each method by means of experiments made simultaneously on different plans. This task is incumbent on the national observatories, and if the investigation is commenced at once, meteorologists, if they bring their results with them to the Vienna Congress, may take up the question with the hope of solving it. But we must not simply confine ourselves to the future. We must think of the past, and of the long series of observations it has handed down to us. With this view it is necessary to determine exactly the correction due to each mode of exposing the thermometers.

* The proceedings of the Meteorological Section of the meeting at Bordeaux are so closely related to those of the Meteorological Conference at Leipzig that it seems allowable to introduce them here, although this publication, as will be seen from its form, was not addressed to the latter Conference.

Maximum and minimum thermometers, p. 17.

4. Rutherford's Minimum thermometers, with uncoloured spirit, graduated on the stem, and compared in the shade with a good mercurial thermometer, are good instruments and will work a long time.

The Maximum thermometer called Walferdin's is almost irreproachable when in use; but it has the disadvantage of being easily deranged during transit from one place to another. It is true that it can be very easily set right, but this operation, however simple it may be, requires a certain amount of practice, and must be done in the presence of an expert. Consequently Negretti and Zambra's maximum thermometers are often preferred to Walferdin's; they have no thermometric chamber, and so the mercury never leaves the bulb during transit. On the other hand the thermometer often "pumps," that is to say, its column jerks from one to two tenths of a millimetre; and the verification of its zero is very uncertain in the hands of anyone who is not used to it. At the Paris Observatory we employ only Walferdin's Maximum thermometer, in conjunction with Six's self-registering thermometer, which, notwithstanding its defects, is maintained for its antiquity. In the provincial observatories Negretti's thermometer is always used whenever it has to be forwarded by ordinary means, but the tendency is to replace it by Walferdin's.

Radiation, p. 19.

5. We only know of one instrument capable of accurately measuring the intensity of diurnal and nocturnal radiation—that is Melloni's pile, performing with a single-needle galvanometer, and whose graduation is obtained by arranging in front of its two faces surfaces coated with lamp-black and of known temperatures. One of these surfaces always remains in its place, while the free face of the pile receives the direct action of the radiations it is wished to measure. But this is a laboratory apparatus, whose use is limited to a few observers; it is not an instrument for ordinary observation.

Pouillet's pyrheliometer can only be used for measuring the direct solar radiation; the same may be said of the Rev. Father Secchi's actinometer, which only shows the rays transmitted through glass, if its case is closed in front by a pane of glass, or, in the contrary case, is influenced by the agitation of the air.

The black-bulb thermometer, placed in an exhausted glass tube, has the advantage of being as easily read as the ordinary thermomcter, and giving the radiation arising from the whole of the sky. It must have *a complete view* of the sky; every portion that is masked from it produces a diminution of the total result. It appears to us necessary also to place an ordinary thermometer, also *in vacuo*, by the side of the black-bulb thermometer. The size of these instruments has a slight influence on their readings. They should therefore be first compared with a standard, or, better still, with the indications of the thermo-electric pile, in order to determine their correction.

Thermometers in a vacuum cannot be used for measuring nocturnal radiation; obscure heat does not pass easily through glass, it is the envelope alone that radiates, and we fall into the disadvantages presented by thermometers without an envelope; the movement of the air modifies their indications. The thermo-electric pile, giving almost instantaneous readings, is alone efficient; we think that this alone can be used as a starting point and a normal instrument for correcting ordinary observing instruments, and for determining the trustworthiness of their indications. This is an investigation which is yet to be made.

Earth temperatures, p. 19.

6. The question is twofold:—

From the general point of view of the Physics of the globe, we must descend to as great a depth as the funds permit, to 30 and 50 metres and more, if possible, according to the nature of the soil. M. Becquerel's electrical thermometer seems to us to be the only one applicable to this case. It would be desirable that sets of this apparatus should be established in each of the principal geological regions, from the north to the south of Europe.

From the point of view of meteorology properly so called, a thermometer placed on the surface of the ground would suffice, and others placed at depths reached by the ploughshare or by the roots of plants. At the depth of a metre thermometers with stems rising above the ground, and capable of being read directly, already require difficult corrections, unless provided with a second

stem without a reservoir, and graduated like a thermometer. Again, the temperature of the surface of the soil differs greatly according to its nature, and the indications of this kind are as special as those of climate; the observations should therefore be multiplied.

7. The psychrometer is a good instrument, if not absolutely good, in the hot season, or in hot countries. But during the cold of winter it wants sensitiveness, and becomes inferior to the most ordinary hair hygrometer. Hygrometry, p. 34.

The hair hygrometer, as it is now made, is not a trustworthy instrument; the hair is stretched by too great a weight, especially when the weather is becoming dry again, and consequently it deteriorates rapidly. Arago has remedied this defect by employing three hairs attached to the same pulley at their lower end, and fixed at their upper extremity to the ends of small levers which allowed a compensation for their unequal extension. This very simple expedient allowed of the use of an instrument, which cannot in all cases be replaced by the psychrometer.

On this point, as on all others, it would be necessary that, in each country, the instruments employed should be submitted to a regular test, so as to preserve the comparability of their indications. The best means for checking hygrometry is always chemical analysis, in laboratories suitably furnished, or else Regnault's condensing hygrometer, in which the silver cup may be replaced by a small tube of bluish-black glass.

8. Sixteen points would seem to us sufficiently definite for indicating the direction of the winds. As to their velocity, it is to be hoped that the use of anemometers will be generally adopted. Robinson's anemometers, furnished with a simple kilometric meter, formed by a series of toothed wheels, and read at the hours of ordinary observation, are moderate in price and give the mean velocity of the wind, in the interval between the observations, with sufficient approximation. If they are erected in a very open place it is not necessary to fix them at a great height, but at the principal observatories it would be well to set up several of these instruments at different heights, in order better to determine the variation of the velocities, from the surface of the ground, to the most elevated accessible regions of the atmosphere. An erroneous interpretation of the law of propagation of velocities, from one stratum to another, has given rise in France to lamentable accidents. Two of these instruments are erected, or are ready to be erected, at Montsouris. One has been placed by M. Deville at the top of a mast 29 metres high; a second will be placed at 3 metres only above the ground, at a distance sufficiently removed from every obstacle to the progressive movement of the air. Direction of wind, pp. 36, 37.

We prefer wind roses, with the velocities introduced, to the mean direction of the wind calculated by Lambert's formula, but we should not make any objection to introducing both methods if meteorologists expressed a wish for them. In cases where the velocity is recorded by an anemometer, it seems to us that the wind denoted by zero should not be counted. Very gentle breezes are generally quite local, and show rather the vertical movements, which are important subjects of study, than the movements of horizontal translation.

9. From a telegraphic point of view, it is desirable that the scale adopted should only consist of a single figure for each degree of force, that it should go, for example, from 0 to 9 as a maximum; this scale is adopted by the French navy. For the construction of charts it is rather less comprehensive; we limit ourselves to the scale 0—6, which is half the English scale. But there would be no serious objection to the adoption of any scale which should unite the majority of votes. Scale of wind force, p. 38.

10. We think that the anemometers mentioned in No. 8 would be of great use, and if the same unit cannot be at once employed in all countries, we would only ask that uniformity should be introduced into all publications of an international character, the arrangement of which by the Congress seems to us to be most desirable. We shall revert to this in No. 22. Velocity of wind, p. 39.

11. The simplest rain gauge, and that most used in France, is the one recommended by the Scientific Association, whose circular funnel has a surface of 4 sq. decim. At the observatory the vessel for receiving the water is placed in a pot sunk in the ground, so that the rim of the funnel is nearly at the mean height of the grass, 10 or 12 cm. above the bare ground, and in a very open Rain gauges, p. 39.

space. At the normal schools, and similar stations, Babinet's udometer is employed, enclosed in a wooden box, the top of which is formed by the funnel, so that, by placing a lamp inside in winter, the snow adheres to the funnel and melts as it is received. The height of the top of the instrument above the ground is in this case about 2 metres.

At the observatory the rain gauge is read at all the hours of observation, but, in the monthly summaries, all the rain which has fallen until 7 or 8 a.m. is added to that of the day before.

Separation of rain, snow, &c., p. 40.

12 and 13. We think so. They are so many distinct signs of the condition of the atmosphere, and are easily noted by means of conventions independent of difference of language.

Thunderstorms, p. 41.

14. With regard to the first point, we think both should be done; as to the second, the regular observation of thunderstorms in France has shown that thunder is not heard above 5 or 6 leagues, while lightning may be perceived at a distance of more than 30 leagues, The so-called heat-lightning is generally a sign of distant thunderstorms; on this account it should be noted, specifying the direction of the horizon in which it appears.

Evaporation, p. 41.

15. Piche's vaporimeter appears to give the most comparable results. The colour of the paper, whether white, blue, red, or green, has a very marked influence on the results when exposed to the sun. In the shade, the differences decrease greatly, and when white paper is used the results are almost identical, either in the sun or shade. We think it best to place the vaporimeter, furnished with a disc of white paper, under a little screen which shelters it from sun and rain. That is our normal vaporimeter, but the observatory would be inclined to establish vaporimeters conformably to those adopted in other observatories, and as far as possible in identical conditions, so as to compare their indications. This step might usefully be made with respect to all meteorological instruments, and be adopted in the principal observatories, at least temporarily.

Proportion of cloud, p. 41.

16. The employment of tenths of clear, or cloudy, sky would require the use of two figures, which for telegraphic reports would be a disadvantage. The symbols engraved by the Meteorological Society for its "Nouvelles" only go from 0 to 4. Before engraving the symbols for the "Bulletin mensuel" of the Montsouris observatory, we shall wait until meteorologists have made known their ideas on the matter. The same may be said of the other symbols proposed to represent the various meteorological elements.

Extra observations, p. 42.

17. We think it useful to extend the field of regular observations as much as the resources of the various meteorological establishments allow. Ozone and atmospheric electricity should be included, but up to the present time atmospheric electricity has not been taken up at Montsouris, as the official and definitive constitution of that observatory dates only a few months back. Although this element enters into our programme, we are unable at present to give any fixed opinion about it.

Could not the Congress also give its attention to observations of Sea Temperature, and the direction and velocity of Marine Currents? These observations are regularly made on several points of the coast of Europe, and especially of France, but they are generally ignored, because they are not yet regularly introduced into the usual meteorological publications.

Uniform times of observation, p. 20.

18. This would be very desirable, but if this result cannot be obtained, there should be at least a certain number of similar hours, in each of the series adopted by the various establishments, to serve as stand points.

Inspections and verifications, p. 23.

19. We think so, and we believe it would be one of the greatest advantages which can possibly be realized in meteorology.

Calculation of means, p. 25.

20. The most convenient periods for us are the day, month, season, and the year, but especially the day, which forms the basis of all discussions. The distribution of temperature and rain, for instance, in the succession of days of a year exerts a decisive influence on the crops, and this action is difficult to recognise in periods, however short they may be.

We think it necessary to publish the daily means every month, and then the commencement of the year is only of secondary importance. The meteorologist will group his observations in the manner most conformable to the object

of the study which he has undertaken; the essential point is that these comparable elements should not be wanting.

The calculation of means varies according to the districts. The chief observatories have at their disposal multitudinous observations made at different hours of the day, and possess a more or less numerous staff, which is not found at stations of the second order. Maximum and minimum thermometers greatly simplify the work; it would therefore seem useful to employ them more generally.

21. The series of observations made at observatories or meteorological stations are of very unequal duration, and embrace very different periods. It does not appear to us prudent to compare them together in bulk, and without discussion; this would be running the risk of grouping, for two different stations, years very unlike in character, and so falsifying the climatal relations of these two stations. Calculation of normal values, p. 28.

There is scarcely a country in Europe where at least one regular and trustworthy series, dating back almost to the time when meteorological observations were begun, cannot be found. These series might usefully serve as a starting point for the discussion of all the rest. Being given a station belonging to a district for which there exists a normal series, we should think it desirable to compare the means of this station, not with the whole of the normal series, but with the number of years of this series which correspond to the observations at the station in question. The two specimen diagram sheets published in the "Bulletin mensuel de Montsouris" for the year 1865–7 have been constructed in accordance with this idea.

For Paris the unbroken curve represents the mean monthly temperatures observed during these years; the dotted curve shows the march of the normal mean temperatures deduced from 60 years' observations at Paris. For all the other stations also the unbroken curve represents the monthly means observed at their dates, but the dotted curve exhibits, not the normal mean temperatures for Paris, but the monthly means for Paris obtained for the same time as those for the stations.

In this comparison we must not embrace points which are too far asunder, or too close to each other, but comprised in dissimilar climates; the work must be guided by an attentive discrimination; but the introduction of years into the comparison which exist in one of the terms, but are not found in the other, would appear to us at least as fallacious.

22. This would certainly be most desirable and most profitable to science. Not only should the documents be inserted in a definite order, but the form itself should be determined by common consent. In forming a meteorological library, nothing is so troublesome and disagreeable as the extreme variety of the dimensions adopted in the publications of different countries. The changes experienced by the Paris Observatory during several years, and from which it had apparently just escaped when the death of M. Delaunay occurred, have produced modifications in the form of its publications, the general tendency of which has been to bring them to one size, Royal quarto. But whatever be the decision arrived at by the Congress of Vienna, we are quite disposed to conform to it. Uniformity of publications, p. 29.

We accept completely the terms of the above-stated question, but at the same time we should like to go beyond it, in taking up the question of publications having an international character. We should like to see the care of these publications divided among the various establishments of the first order.

We may be permitted here to cite some of the opinions examined by the Congress at Bordeaux, without wishing in any way to anticipate the decisions of the Vienna Congress, and more especially without transgressing the personal courtesy due to the honourable *savans* whose names we quote.

The following is the last paragraph of M. Rubenson's printed letter:—

"Is it not desirable that M. Buys Ballot's excellent tables on the state of the atmosphere over Europe should be completed and extended to such a degree as to allow of their being used as a basis for all investigations of the "Non-Periodical Variations" over the whole of Europe, by means of special preparatory works of the meteorologists of other countries?"

We share this wish, and for our part would adhere to it, adding that it would

be equally desirable that this same establishment, or others, were charged with the extension of the network to Asia, America, and the Oceans too.

Another question of a similar kind has been raised by M. Rubenson :—

"What measures should be taken for obtaining, in the shortest possible time, general charts of meteorological elements for which none exist at present, and for improving and completing those already constructed?"

The Observatory of Paris had undertaken an Atlas of the General Movements of the Atmosphere over the surface of the Atlantic and of Europe. Circumstances which need not be recalled here have interrupted this work, which has been partially resumed by M. Mohn. Instead of leaving to this useful work the somewhat personal character which it has maintained up to the present time, notwithstanding the co-operation of different meteorologists, it cannot be doubted that its adoption by a higher authority would ensure its duration, while furnishing it with more powerful means of action. Without in any way trammelling the free development of individual efforts, it seems to us that if meteorologists could agree as to the grand works that the science demands, and as to their division among the establishments disposed to undertake them on the general basis decided by the Congress, the science would advance with rapid strides. Each work effected would, moreover, open out new fields, and the congresses becoming periodical, the general work could each year be made to conform to the new conditions arising from the progress made.

Exchange of publications, p. 31.

23. We shall content ourselves by stating here what is done in France. The Observatory is allowed to send the "Bulletin International" post free over French territory, but this privilege ceases at the frontier. The numerous meteorological documents which are sent from France to the Observatory are enclosed in an envelope addressed to the Minister of Public Instruction, and bearing the words "Service Météorologique;" these documents are sent free to the Observatory. The transmissions from the Observatory to its correspondents in France are sent free in the same way, provided the address is not Mr. So and So, Prefect, Director, or President; but simply "The" Prefect, Director, or President. If the "frank" stops at the frontiers, it is because it is not recognized by the other states; but free transmission being given for meteorological despatches by telegraph, the same might also be conceded, as regards the post, under certain conditions, which may be easily imagined and carried out. There is the consular route also, of which we occasionally make use.

Establishment of central offices, p. 31.

24. Such an establishment exists in France, and it enters into the traditions of the country there, but it only wishes to possess a moral influence over its correspondents; it limits itself to furthering their works and to contributing to give them greater homogeneity and to make them known. The bonds which link the stations to the central establishment are quite voluntary, and that is sufficient.

Weather telegraphy, p. 32.

25. We shall content ourselves with reproducing here M. Fradesso da Silveira's reply to this question in his printed letter:—

"It appears to us that the exchange of meteorological telegrams becomes indispensable; but this interchange will not produce the results which might be expected, until all countries take suitable steps for preventing delay in the transmission of bulletins."

An inspection of the Daily Bulletin of the Observatory shows how far this condition is from being realised.

Plans for Congress, p. 34.

26. The Congress of Vienna may take place by the voluntary meeting of meteorologists of different countries. They will have sufficient standing to discuss the questions relative to the future of their science, and to enter into personal engagements to carry out the conditions prescribed. The solutions proposed by this assembly, even if it were merely of a private nature, entirely without any official connexion, would no doubt have great authority with the respective governments; we think, however, that under these conditions the meeting would lose much of its efficacy.

In the majority of European states, meteorological institutes or establishments are founded and maintained by the governments; and whatever authority they have in their own country, the directors of these establishments are responsible, both for the works which are entrusted to them, and for the resources, in materials and staff, placed at their disposal.

The deliberations of the Congress may, and ought to, bring about modifications in the nature and conduct of the works undertaken; financial and other engagements may, and should, result from them, either by the proposal of new units of measure, the origination of publications of general utility, or the request for free transmission, by telegraph or post, on a more extensive scale. If questions of such a nature may be raised it seems that the Governments interested ought at least to be invited to represent themselves officially. They alone, in fact, can give to the Congress a really International character, and are able to give a guarantee that the necessary steps shall be taken to give effect to its deliberations.

It seems therefore necessary that the organizers of the Congress should obtain the official adherence of the Governments to the projected meeting.

That if these steps are thought unnecessary or inopportune, the first Congress, while maintaining a private character, should endeavour to establish an International Association of acknowledged public utility, having regular sittings like the International Geodetical Society, and like it appointing a permanent working committee, whose duty should be to prepare for future meetings, and to watch over the execution of former resolutions. Whatever amount of activity may be displayed by the meteorologists assembled at Vienna they can only sketch out the preliminary groundwork of their future operations. The organization of a science, requiring such great co-operation and uniformity of method, cannot be the work of a day; it requires permanent and prolonged exertion.

LONDON:
Printed by GEORGE E. EYRE and WILLIAM SPOTTISWOODE,
Printers to the Queen's most Excellent Majesty.
For Her Majesty's Stationery Office.
[9088.—750.—2/73.]
[P. 1981.—250.—2/73.]

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